# INVENTING BIOMETRY, INVENTING "MAN": <u>BIOMETRIKA</u> AND THE TRANSFORMATION OF THE HUMAN SCIENCES

By

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FOR MY PARENTS

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The dissertation is a historical investigation of the creation of the science of biometry. Its main goal is to inquire about the role of identity and narrative in the production of knowledge about human beings. Accordingly, the dissertation pays equal attention to the historical and scientific writings of the early biometricians, and examines the way that those discourses contributed to the fashioning of an identity for both the biometricians and the objects of biometric science.

The investigation reveals that these identities were fashioned in a central tension over the status of the individual. First, an analysis of the historical writings of the biometricians focuses on the tension

between the unrelenting positivism of the biometric approach (and the inherent desire to erase the individual researcher from the knowledge producing process) and the essentially Romantic character of their narratives (and the Romantic desire of the individual to make a glorious attempt to transform himself). Second, an analysis of the scientific writings of the biometricians reveals a parallel tension between the statistical imperative of the biometric approach (and the desire to remove the unpredictable individual from the equation of knowledge) and the biometrician's insistence that statistics remain a practical science (which reflected the desire to contribute to the solution of certain social ills that would radically alter the lives of a multitude of individuals).

Finally, the dissertation examines the ways in which the internal tensions of the biometric narrative worked themselves out in the scientific practice of the biometricians. The analysis concludes that the biometricians became obsessed with what they considered to be an evolutionary struggle between pedigrees of difference, and tended to produce objects for study that were ever greater reifications of socially constituted notions of normalcy and deviance.

# INTRODUCTION

"History teaches us..."

"It is a biological fact..."

When these two phrases are written or uttered, whether in the somber prose of a widely read editorial column or in an animated coffee-house discussion, they usually preface some sort of definitive expression of the true nature of the human condition. Why do so many people turn to these two phrases when, tiring of argument, they attempt to end all discussion with an appeal to unquestioned and unquestionable authority? It is, I believe, because each phrase appeals to a mode of discourse which currently enjoys a privileged place in the production of knowledge about human beings.

It would be interesting to ask how these two forms of discourse (the historical and the biological) achieved the lofty positions they enjoy, but such a task would require resources and capabilities currently beyond my grasp. Instead, I propose to examine one episode in the production of knowledge about human beings--the formation and development of the science of biometry (circa 1890-1932)--in which both historical and biological discourses played an important role.

I have chosen biometry for a number of reasons. First, as the subtitle of its publication organ Biometrika: A Journal for the Statistical Study of Biological Problems suggests, the authors of biometric discourse were in part attempting to create a new language to articulate and solve the problems of "biology." Second, the major concern of the biometricians was with the "problem of evolution in man," and they considered the problem to be historical. Third, they published a significant number of historical memoirs concerning the origins and development of biometry and its role in the history of science, side-by-side with the scientific material on the pages of their journal. Fourth, the creation of the science of biometry came at a time when the primary responsibility for defining "Man" had passed from theology to the human sciences. Finally, historical knowledge about biometry has been produced by a fully mature body of historical writing. These five circumstances make it possible to examine both the historical record and the historiography of biometry, with an eye towards gaining an understanding of the ways in which knowledge producing discourses work to fashion identities for both the researcher and the researched.

Such a goal requires a concern with the role of rhetoric in history and in science. In that sense, this

inquiry can be seen as participating in the so-called "linguistic turn" that has occurred in the practice of history in recent years. There has, however, been so much debate (and even more confusion) over what this might entail that it will be worthwhile to articulate how I have attempted to proceed in this particular work.

Much of the focus on language, literary theory, and rhetoric in historical studies has been characterized as part of a movement to revitalize intellectual history, both intellectually and institutionally. In this context, emphasis on rhetorical analysis and other forms of literary theory in historical writing is understood as a reaction to the dominant position that social theory enjoyed for most of the sixties and seventies.[1]

My project in no way attempts to revitalize or redeem anything. It is, instead, an attempt to make a small contribution to our understanding of the way that knowledge of nature, and of the past, is created and disseminated. As such, it seeks to participate in a long and rich discourse identified simultaneously with intellectual history, the history of science, and the philosophy and sociology of knowledge. Accordingly, most of the extraordinarily energetic dialogue (which has frankly generated more heat than light) over the value of literary theory in historical studies--focused as it is on the likely effects of such a turn of events

on the discipline of intellectual history--seems of little relevance, and of even less help, to this project.

One element of a recent critique does, however, seem worth mentioning, because it speaks directly to a difference between the approach taken in this project, and the general formalism that has characterized much of recent historical writing informed by literary theory. Russell Jacoby has pointed out what he calls "a fundamental irony" in the formalism that characterizes many of these works. Commenting on the work of Hayden White, but making a point that he clearly believes to apply generally, Jacoby notes that White "consistently rejects as misguided efforts to imitate scientific neutrality and objectivity. Yet he bills his formalism as more objective, almost more scientific, than [other] approaches."[2]

Jacoby is being simplistic, and perhaps disingenuous, when he attempts to characterize all formalist historical writing as motivated by naive and ironic notions of scientific objectivity. But his observation does serve to raise the question of the purpose of formalism. It is valuable to remind historians, as Jacoby does, that formalism will not eliminate the subjectivity inherent in writing about the past. It is probably also useful to note, as Jacoby

does, that formalism is not much of a remedy for the unbearable prose of the sociological discourse that has all but strangled whatever "wider" audience that academic history may have once enjoyed.

These points are not valid objections to the use of formalism in a certain genre of historical writing. Formalism has allowed theoretically oriented historians to create a space in which to "re-think" their relationship to the historical record and to consider the epistemological implications of intertextuality to the writing of history.[3]

However, while the analysis that I present in this project is informed by literary theory and formalist texts in ways that I will try to make clear, the aim of my project is different. I wish simply to tell a story about the role of particular rhetorical strategies in the creation of the science of biometry, and of statistical knowledge about human beings, in the context of late-nineteenth and early-twentieth century British culture. Towards that aim, I will eschew formalism and banish technical terms to the footnotes where they will undoubtedly survive my attempt to marginalize them.

There have been several books published recently that are concerned specifically with the role of rhetoric and writing in science. Among those that have received the most notice are works by Peter Dear, Alan

G. Gross, Greg Myers, and Lawrence J. Prelli.[4] Reaction to these works has been swift and heated. A recent review by Trevor Melia outlines the central themes of objection. While urging a more precise usage of the term rhetoric, Melia asserts that "While the term rhetoric almost invariably refers to 'persuasion,' it usually fails to disambiguate three different orders of persuasion, namely, rhetoric as the act of persuading, rhetoric as the analysis of such acts, and rhetoric as a 'world view.'"[5]

It is the use of rhetoric in the third sense that, when applied to science, seems to create the greatest uproar. Melia asserts that while most would grant that "situated acts of scientific discovery, science policy, science popularization, priority claims in science, and so forth, are susceptible to rhetorical scrutiny of the first two varieties," he cautions that "rhetoric of the third kind has pretensions of an entirely different order."[6] The "pretensions" harbored by the practitioners of this rhetoric of a third kind are their beliefs that they can "assault the scientific fortress" by denying the distinction between "internal and external studies of science and between the context of discovery and the alleged non-context of justification."[7] Melia, and he is not alone, seems genuinely offended that these vandals are not concerned

that their "assault subverts the already posited distinction between acts of persuasion and the analysis thereof."[8]

The imagery of fortress walls demarcating the province of science identifies the issue usually at the center of all the heated debate that seems to constrict any discussion of the role of language in science--the question of the demarcation of science. The subversion of distinctions between internal and external, discovery and justification, text and context, representing and intervening, practice and discourse, and persuasion and analysis is seen as a threat to the uniqueness of science.

Unfortunately, any hint of a threat to those distinctions is also automatically seen as a declaration of uncompromising relativism and is immediately read as a statement of an "anti-realist" philosophy of science. For example, Melia identifies Greg Myers as being "like most poststructuralists," and "writing as if the debate between realist and antirealist philosophers has been resolved in favor of the latter."[9] Now it may be the case that Myers is "like most poststructuralists," whatever that might mean, and he may in fact believe that the debate between realist and antirealist philosophers has been resolved, but I do not see that he would have to change the arguments in his book

significantly if he underwent a sudden conversion to philosophical realism. There would still be room for an inquiry into the role of realism as a kind of rhetoric in the creation of scientific knowledge. In short, I am not at all persuaded that it is necessary to deny the existence of the elephant in order to argue that the rhetoric of the blind man (who claims to know the elephant) is worthy of historical analysis. In any case, the experience of researching and writing a history of the way in which historical and scientific genres of writing were used to fashion a mathematical science of "man" has eroded my ability to willfully ignore the slippage that occurs between such distinctions.

The present inquiry begins with an analysis, in Chapter 1, of the two major strains of historical writing about biometry. Specifically, I argue that previous historical understanding of biometry has been framed by the relationship of two metaphors, synthesis and controversy, and a concern for a single philosophical problem, the demarcation of science.

The first strain of historical writing about biometry constituted it as a controversial episode in the history of modern evolutionary biology, but made the controversy metaphor subordinate to the metaphor of synthesis. Exemplified by William B. Provine's The

Origins of Theoretical Population Genetics (1971), this line of narrative identified biometry as the statistical study of variation. In this narrative, the biometricians, two strong-willed individuals, Karl Pearson and W.F.R. Weldon, made significant theoretical and methodological contributions to the science that would come to be known as theoretical population genetics, a key component in the "modern evolutionary synthesis." Unfortunately, their strong personalities led them into an emotionally charged controversy with the developers of the other major component of the synthesis, Mendelian genetics, and delayed the synthesis.

By evoking the metaphor of controversy, but subordinating it to the metaphor of synthesis, Provine fashioned a knowledge-producing narrative that historicized the developments that led to the modern evolutionary synthesis. This narrative depicted modern scientific knowledge as developing along a specific path, but made that development contingent on things as unpredictable as personal emotions. The unpredictable (and historicizing) human element was implicitly identified as a factor "external" to the development of scientific knowledge.

The second strain of writing that shaped our understanding of the history of biometry is exemplified

by Donald MacKenzie's Statistics in Britain (1981). In MacKenzie's narrative, the metaphor of controversy was exploited to subvert the metaphor of synthesis and to challenge explicitly the notion of the inevitable progress of science.

MacKenzie's narrative identified biometry as a goal-oriented research program intimately tied to the development of eugenics. Analysis of those goals revealed that the biometric program served to further the interests of the professional middle classes of late-nineteenth-century Britain. MacKenzie's analysis of twin controversies--the first between the biometricians and the Mendelians, and a second between Karl Pearson and F.Y. Edgeworth--explicitly challenged the notion of internal and external factors that served as the demarcation criterion implicit in Provine's narrative. In MacKenzie's narrative, not only the path of scientific development was historically contingent, but also its destination.

The analysis of the historiography of biometry in Chapter 1 concludes that, for all its superficial diversity, our entire understanding of the history of biometry has been framed in the context of a debate over a single question inherited from the philosophy of science, and by two metaphors which have constituted and controlled the historical identity of the biometricians.

In short, our knowledge of the history of biometry is fictive, in the sense that it has been fashioned through narrative by specific uses of metaphor.

Chapter 2 explores the possibility of writing a history of biometry for its own sake, that is, a history free of the overriding concern for demarcation criteria and from controlling metaphors. The effort produces three fragments of narrative from fresh research in the archives related to biometry. These narrative fragments explore, in much greater detail than has previously been published, the story of the research institute that Karl Pearson tried to build at the University of London, the story of the creation and dissemination of Biometrika, the official journal of biometry, and the story of the larger group of biometricians that regularly contributed to the journal.

The fragmentary nature of these narrative strands, however, underscores the necessity of a central metaphor and a central story line. Without a central concern and a guiding metaphor, such strands of narrative are merely an exercise in antiquarianism. The rhetorical strategies underlying Provine and MacKenzie's historical writing must not, therefore, be understood as unfortunate aberrations, but as a necessary component of the production of historical knowledge.

The rest of this study proceeds to investigate biometry as a knowledge-producing narrative, and makes "writing" the central metaphor for its story, a story about the role of narrative in history and in science. Accordingly, Chapter 3 deals with the role of narrative in the creation of the subject of biometry, that is, in the creation of the identity of the new science and its practitioners. Specifically, Chapter 3 traces the creation of those identities through the numerous historical writings of the biometricians, which they published alongside their scientific treatises in Biometrika. It then contextualizes those historical writings by making them interact with the narrative strands presented in Chapter 2, and with the general historical narrative of late-nineteenth-century Britain, to produce an integrated history of the creation of biometry as a subject.

Chapter 4 analyzes the structure and elements of biometric narrative more closely through an examination of Karl Pearson's "Walter Frank Raphael Weldon, 1859-1906," his tribute to the co-founder of Biometrika. The analysis reveals a basic tension between conflicting desires inherent in the historical writing that fashioned the identity of the subject of biometry. The tension is revealed by juxtaposing the unrelenting positivism of the biometric approach (the desire to

remove the individual researcher from the knowledge produced) and the essentially Romantic character of their narrative (the desire of the individual to make a glorious and heroic attempt to transform himself).

Chapter 5 turns attention to the scientific treatises which fashioned the objects of biometric study, for example, the mid-parent, the modal-individual, and true-breeding pedigrees. The analysis of those scientific treatises uncovers a tension identical to the one found in biometric histories by juxtaposing the statistical imperative of the biometric approach (the desire to remove the unpredictable individual from the equation of knowledge) and the insistence that statistics remain "practical" (the desire to contribute to the solution of certain social ills that would radically alter the lives of a multitude of individuals).

Chapter 6 examines in greater detail the process of producing the objects of biometric discourse to see how the internal tensions of biometric narrative worked themselves out. The analysis reveals, first, a tendency to produce objects that are ever greater reifications of socially constituted notions of normalcy and deviance, and second, a tendency, when attempting to provide translations between biometric and more traditional discourses, to substitute the new objects for the old.

## Notes

1. Two American Historical Association Forums provide an excellent introduction to the debate, both pro and con, over the redemptive role of literary theory in historical practice: David Harlan, "Intellectual History and the Return of Literature," American Historical Review 94 (1989): 581-609; and Russell Jacoby, "A New Intellectual History?" and a response by Dominick LaCapra, "Intellectual History and its Ways," American Historical Review 97 (1992): 405-439.

2. Jacoby, "A New Intellectual History?" p. 413.

3. See for example, Dominick LaCapra, Rethinking Intellectual History: Texts, Contexts, Language (Ithaca: Cornell University Press, 1983).

4. Specifically, the works are: Peter Dear, The Literary Structure of Scientific Argument: Historical Studies (Philadelphia: University of Pennsylvania Press, 1991); Alan G. Gross, The Rhetoric of Science (Cambridge, Mass./London: Harvard University Press, 1990); Greg Myers, Writing Biology: Texts in the Social Construction of Scientific Knowledge (Madison/London: University of Wisconsin Press, 1990); and Lawrence J. Prelli, A Rhetoric of Science: Inventing Scientific Discourse (Columbia: University of South Carolina Press, 1989).

5. Trevor Melia, "Essay Review," Isis 83 (1992): 100-106, p. 100.

6. Ibid., pp. 100-101.

7. Ibid., p. 101.

8. Ibid.

9. Ibid., p. 103.

# CHAPTER 1
# HISTORIANS TELL TALES

The historian begins each new research project by reading the works of other historians. Such a survey of the existing historiography is undertaken to determine whether or not fresh research will enable the historian to pose and answer new questions, and to contribute to a fuller and deeper understanding of the subject and its historical context. If, after completing the historiographic survey, the historian believes that fresh research will be fruitful, (s)he writes a historiographic essay, carving out a niche for his/her own work in the field of historical discourse about the topic.

The metaphor of the niche is worth examining. When a given species is said to occupy a niche in an ecosystem, it survives and thrives precisely because it has made a place for itself _within_ a given structure. When historians find a niche within the historiography of a topic, they too fit into a pre-existing structure of sorts--a structure of meaning, fashioned and controlled by central metaphors, which define and identify the object of investigation.

The work the historian produces becomes a "contribution to the literature" which, regardless of its revisionist interpretation, supports and extends the assumptions contained in the metaphors which identify the historical object and structure the discourse surrounding it.

The work of history produced here is, among other things, a historical study of the science of biometry, and there is, of course, an existing historiography. In a review of a grant proposal for this project, one historian wrote: "I would have thought that nothing more could be said about the biometricians that hadn't already been said." There is, however, always more to say or, more accurately, there is always a different story to tell. For in spite of the considerable quantity of historical writing about biometry and the biometricians, there are essentially only two stories that have been told, and they form two sides of a single debate. Each story is dominated by the same two metaphors--synthesis and controversy, and both stories take part in a contentious discourse about the nature of the development of scientific knowledge.

What distinguishes the two stories is the priority given to the two metaphors. In the older of the two stories, the metaphor of "synthesis" is central. Written from the point of view of an author who has the

benefit of a century of scientific progress, the story defines biometry in relation to the "modern evolutionary synthesis" in which the Darwinian concept of gradual evolution by natural selection (which the biometricians defended as central to any understanding of evolution) was, after a period of controversy, combined with concepts and insights propounded by the Mendelian geneticists to create, through further research, the modern theory of evolution. It is a story of a trip along the often treacherous path to the discovery of truth.

In the revisionist history of biometry, the metaphor of synthesis is present only in its implicit negation, and the metaphor of "controversy" becomes central. Here biometry is identified as one half of a scientific controversy (the "biometry-Mendelism controversy") which is examined as a case study in the sociology of knowledge. Written in the measured prose of sociological analysis, the revisionist story focuses on the links between biometry and the eugenics movement, identifying both with the social interests of the professional English middle classes. It is a story about the social construction of knowledge.

Both strains of historical writing participate in a discourse about the nature of change in science. The narrative that privileges the synthesis metaphor tells a

story about the inevitable progress of scientific discovery. By acknowledging that controversies occurred along the way, the authors guarantee that their tales are genuinely historical, and acknowledge that the development of science may be bumpy and historically contingent. But by subjugating controversy to synthesis, they maintain that science is progressive and driven by an internal logic. The narrative that privileges controversy explicitly challenges the notion of scientific progress by demonstrating the social contingency of scientific development. A close reading of exemplars from each genre will illustrate the point.

## Biometry and the Journey to Modern Synthesis

William B. Provine's The Origins of Theoretical Population Genetics exemplifies the history of biometry that concentrates on the role of the biometricians in the creation of the modern evolutionary synthesis. As the title suggests, the story is written from the point of view of an author who enjoys the full benefits of the most modern understanding of the evolutionary process. The story looks back to find the origins of that knowledge.[1]

Provine finds the origin of theoretical population genetics--the science that, in Provine's view, produced our modern understanding of evolution--to be "best understood as a product of the conflict between two

views of evolution which were eventually synthesized."[2] The conflict began with the publication of the first edition of Charles Darwin's Origin of Species in 1859 and ended, in the early 1930s, with the creation of a new synthetic theory that followed from the work of R.A Fisher, J.B.S Haldane, and Sewall Wright.

The two conflicting views of evolution hinged on two separate but related points: the degree of continuity in the evolutionary process, and the efficacy of natural selection in creating new species. One view, supported by Darwin, envisioned evolution to proceed by the continuous and gradual modification of species through the agency of natural selection acting on the small variations that exist between every individual in a population--the kind of variation which Darwin termed "individual differences." The alternative view, attributed to both T.H. Huxley and Darwin's cousin Francis Galton, envisioned evolution by the discontinuous and rapid modification of species due to the occasional appearance within populations of a qualitatively different types of variation known as "single differences," "sports," or "saltations." In the latter view, natural selection alone was not, and could not be, responsible for the creation of new species.

Provine's explanation of why Huxley and Galton broke with Darwin relies on an implicit theory of

scientific judgment and human action which privileges the internal consistency of scientific logic and scientific theories. For example, in Provine's narrative, Huxley's commitment to the notion of discontinuous evolution brought about by the appearance of saltations is explained by the fact that it "harmonized" with the evidence of the geological record which failed to show a complete and continuous transmutation of species.[3] Similarly, Francis Galton's preference for saltation theories is explained by his belief in the principle of regression and the stability of sports, and both of those beliefs are attributed to observation and scientific reasoning.[4]

Galton's reasoning is summarized as follows. After observing that the general character of populations normally remained constant over generations, despite the fact that individual differences constantly occurred within the population, Galton concluded that such constancy could not be maintained if exceptional members of the population produced offspring equally or more exceptional than themselves. Further, the constancy of the population's overall character meant that the exceptional members must often produce offspring less exceptional than themselves, since the average types would occasionally produce offspring more exceptional than themselves.

Galton extended and quantified this line of reasoning to create a "law of regression." When applied, for example, to stature this law stated that

> the deviations of the sons from P (the median stature of the general population) are, on the average, equal to one third of the deviation of the parent from P, and in the same direction. Or more briefly still: if P+(+/-D) be the stature of the parent, the stature of the offspring will on the average be P+(+/-1/3D).[5]

From that insight Galton derived what Karl Pearson later named the "law of ancestral heredity," which stated that

> the influence, pure and simple, of the Mid-Parent may be taken as 1/2, and that of the Mid-Grandparent as 1/4, and so on. Consequently, the influence of the individual Parent would be 1/4, and of the individual Grand-Parent 1/16, and so on.[6]

For Galton, the implications of this line of reasoning were clear. Selection, though clearly active in nature, could not have a lasting impact on the general character of a species because an equilibrium between deviation and regression would soon be reached.[7]

Provine's narrative characterizes Galton's belief that "sports" were the crucial type of variation for evolution as a reasoned solution to another problem in Darwin's theory--the so-called "swamping effect." In Galton's day, the phrase "swamping effect" referred to a logical conclusion drawn from the belief, based on observation, that most hereditary traits were the result of a blending of the characteristics of each parent. Since, by definition, "individual differences"--the

common variation that was the raw material of evolution in Darwin's theory--occurred in every individual, any given population would include a multitude of diverse hereditary tendencies. In a freely mating population, those tendencies would be constantly blended to produce offspring exhibiting diluted characteristics. In such a situation, even the most useful variation would have to occur simultaneously in an enormous number of individuals or it would be "swamped" out of the population before it could be accumulated by natural selection.[8] "Sports" were a possible answer to the swamping problem because many people, including Galton, considered them to be fundamental "changes in the position of organic stability [which] do not blend freely together."[9]

As Provine's narrative unfolds, Galton's investigations constitute the first episode in a dramatic development that leads to the modern synthesis. Specifically, Galton is seen as producing two contributions to that development. First, he began what was to become a long tradition of statistical studies of heredity in natural populations and, second, he crystallized a line of argument that supported, in opposition to Darwin, the notion of discontinuous and abrupt evolution.

Provine's privileging of the internal logic of scientific theories continues in his treatment of the second episode in the drama, the conflict between the biometricians and the Mendelians. In Provine's account

> the conflict caused a split between those who advocated Mendel's theory of heredity and those who advocated Darwin's theory of natural selection. If the Mendelians had worked with, instead of against, the biometricians, the synthesis of Mendelian inheritance and Darwinian selection into a mathematical model, later accomplished by population genetics, might have occurred some fifteen years earlier.[10]

The conflict pre-dated the creation of either biometry or Mendelism. It was based on a continuation of the disagreement between Darwin's notion of continuous evolution by natural selection on the small but abundant type of variation, and the contrary notion favored by Huxley and Galton of discontinuous evolution by "saltations." The biometricians, represented in the story by Karl Pearson and W.F.R. Weldon, defended and extended the Darwinian view, while the Mendelians, represented initially by William Bateson and later by Wilhelm Johannsen and Hugo DeVries, supported the discontinuous view by interpreting and extending the "rediscovered" work of Gregor Mendel.[11]

The powerful personalities of the individual combatants are the important characters in Provine's narrative. Galton remained aloof from a conflict in which both sides claimed him as a founding father, but

Karl Pearson became something of an enfant terrible. Provine describes Pearson as a young man of "great energy and diligence," keenly aware of his own intelligence, who was "quick to criticize the incompetence of others," and to "discredit prime examples of sloppy thinking."[12] A man with incredible breadth of intellectual interest, Pearson described himself as a "buccaneer...with decidedly piratical tendencies."[13] Only the steadying influence of Pearson's colleague, W.F.R. Weldon, enabled Pearson to focus on the work that would result in the creation of biometry.[14]

In contrast to Pearson's fierce but wandering intellect, Provine's Weldon focused on biological questions early in his career. He studied botany and zoology under Ray Lankester at the University of London for a year before moving on to Cambridge where he became the brightest student of the respected morphologist Francis Balfour.[15] Weldon's personality was characterized by an excitement for the theories of Charles Darwin and an eagerness to follow in his footsteps. In 1889, both Pearson and Weldon read and were excited by Galton's Natural Inheritance. Weldon became convinced, according to Provine's narrative, that "the evolutionary relationships which traditional morphology had attempted to demonstrate might be better

demonstrated by appropriate statistical studies of populations."[16]

Weldon, however, lacked the mathematical skills necessary to carry out his vision. He did his best to remedy the situation by studying the great French mathematicians, but he still required help. He found that help in the person of Karl Pearson at University College London, where they were both teaching in 1891. Weldon's enthusiasm was contagious, and Pearson caught the bug. Soon, the two of them were articulating the principles of the new science that would be called biometry, and that would claim Francis Galton as its founding father.

Provine agrees with those who contrast the keen intellects and confident enthusiasm of the founders of biometry to the personality of William Bateson. Bateson was "not very happy and did poorly at school."[17] He was unpopular and overly sensitive to criticism. He was also apparently insecure about his status. While Bateson and Weldon were students together at Cambridge, they were close friends. Years later, however, Bateson complained about having been "made to feel like Weldon's bottle-washer."[18]

Provine describes how Bateson, like Weldon, became disenchanted with the morphological approach to the study of evolution, and turned to the study of

variation. Bateson's study of variation in natural populations, commencing in 1883 and including an expedition to Russia in the Spring of 1886, made him increasingly skeptical of the Darwinian claim that evolution progressed gradually through the actions of natural selection on individual differences.[19] His skepticism culminated in a distinctly non-Darwinian paper on floral symmetry published in 1891.[20]

Provine explores how Bateson found support for his ideas in Galton's work. Bateson too had read, and been influenced by, Natural Inheritance. In 1894, when Bateson published his huge manifesto of discontinuous evolution, Materials for the Study of Variation, Treated with Especial Regard to Discontinuity in the Origin of Species (London: Macmillan, 1894), both Galton and Huxley gave it an enthusiastic welcome.[21] Neither Pearson nor Weldon shared the enthusiasm but, in light of Galton's support, they tried to temper their criticism. Provine cites correspondence in which Weldon let Bateson know privately that he could not conceive of "characters which do not mix [and] are thereby rendered independent of the phenomenon of regression."[22] Meanwhile, Pearson gently asserted that Galton had "misunderstood" the implications of his own work on regression. Pearson argued that the phenomenon of regression did not necessarily eliminate the possibility

of evolution by natural selection accumulating individual differences. If the exceptional variants bred only amongst themselves, then the "norm" towards which the population tended to regress was not an unchanging stable value, but a value constantly changing as natural selection modified successive generations of the population.

In this section of Provine's narrative, he gives the reader a glimpse of the logical way in which the work of Bateson and the biometricians could have complimented one another, but the potential for harmony is quickly dashed as the "personalities" of the characters takes over. Weldon's review of Materials, published in the 10 May 1894 edition of Nature was sharply critical. Essentially, Weldon asserted that both Bateson's ideas about discontinuity in evolution and his method of research were misguided. Further, he indicated that Bateson would have done better to have adopted biometric methods for the study of variation. Bateson, with his book selling poorly, perceived Weldon's criticism as a personal attack and, adopting the persona of an injured party battling an established orthodoxy, launched a series of public attacks on Weldon and biometry.[23]

Provine's account of this episode becomes the model (and primary source) for a line of historiography that

views these attacks, together with the Weldon's and later Pearson's responses, as constituting the public controversy that has become known as the "well known biometry-Mendelism controversy." In the course of that drama, the view supporting discontinuous evolution came to be called "Mendelism," because it was in the midst of the controversy, in 1900, that Gregor Mendel's now famous "laws of heredity" were rediscovered. Bateson and other advocates of discontinuous evolution seized upon them as a theory of discrete inheritance which perfectly complemented their theory of discontinuous evolution. The biometricians, on the other hand, viewed Mendelism as a threat to the proper investigation of heredity and challenged its validity.[24]

As Provine's story continues down the path to the creation of the modern synthesis, through the creation of mutation theories and the eventual development of theoretical population genetics, the biometricians disappear from the narrative. They disappear because the controversy diverted them from the path. The proper path described by the internal logic of scientific discovery was that which led to the combination of Mendelism and statistical studies. Accordingly, the actors in these subsequent scenes in Provine's drama are not the biometricians, who continued to deny the general importance of Mendel's laws, but rather those who showed

how Mendel's laws and a statistical Darwinism could be harmonized.

Specifically, the rest of the tale consists of a series of progressive steps towards synthesis beginning with three essential developments before 1912. First was a series of experiments by William Castle that were interpreted as demonstrating that selection of continuous variations could cause a permanent change in the character of a population. Second was the experimental demonstration provided by H. Nilsson-Ehle and Edward East which seemed to show that a Mendelian explanation could be provided for apparent instances of continuous variation and blending inheritance. The third development came from the lab of T.H. Morgan whose experiments with Drosophila suggested that Mendelian characters might include very small variations.[25]

Three more potentially progressive developments had been achieved, but not exploited, by 1918. The first of these was the working out of the later-named Hardy-Weinberg Equilibrium Principle, which describes the conditions under which evolutionary equilibrium is maintained, that is, the conditions under which changes in gene frequencies and genotypes do not take place. The second was the working out of the mathematical consequences of inbreeding principally by American researchers like H.S. Jennings and Raymond Pearl.

Rounding out this second group of developments was an analysis of the effects of selection prepared by the mathematician H.T.J. Norton and published by R.C. Punnett.[26]

Provine's narrative concludes with the achievement of synthesis brought about through the efforts of R.A. Fisher, J.B.S. Haldane, and Sewall Wright. All three worked to build mathematical models of the effects of selection on gene frequencies. Through their efforts, they synthesized Mendelian heredity and natural selection into the science of population genetics.

As early as 1916, Fisher had begun writing papers whose explicit purpose was the reconciliation of biometry and Mendelism. Fisher agreed with Pearson that natural selection was the primary agent of evolutionary change and that it operated on apparently continuous variations. But unlike Pearson, Fisher believed that Mendelian heredity could produce apparently continuous variation, with the added advantage of preserving a high degree of variability in populations. Years of work culminated in the publication, in 1930, of his Genetical Theory of Natural Selection. Fisher's contribution to the synthesis was to show that the effectiveness of selection depended on the total amount of heritable variation available in a population at any given time.[27]

Haldane worked along the same lines as Fisher, but became convinced that evolution could work much more rapidly than Fisher supposed. His most famous example was the case of industrial melanism in the peppered moth Amphidasys betularia (later renamed Biston betularia). Between 1848 and 1900 a darker form had almost completely replaced the normal version of gray moth in the industrial soot-laden areas of Britain where its darker color would hide it from its predators. Haldane showed that such a rapid spread of a new variety indicated a far more intensive selective effect than previously imagined.[28]

Sewall Wright's contribution was to suggest that evolution was the interaction of systems of genes, and specifically of the random drift of genes caused by inbreeding. Essentially, Wright demonstrated that the creation of new interaction systems within the gene pool could act as an intermediate factor between mutation and selection.[29]

In Provine's story of the progress of scientific development that led to modern evolution theory, the biometricians played a specific and fleeting role. They were the creators of a certain methodology that was to be incorporated into the modern science of theoretical population genetics and modern evolutionary theory, but they did not themselves bring that development about.

Instead they became a historical example of the way in which "powerful personalities...generated such strong personal antagonisms that collaboration, which might have been fruitful, was virtually impossible."[30]

## Biometry, Eugenics, and the Social Construction of Knowledge

The genre of historical writing about biometry that contests the validity of "synthesis" as a metaphor for scientific development is exemplified by a series of publications produced by the proponents of what has come to be called the "Strong Programme" in the sociology of science. The authors of this genre, principally Donald MacKenzie with occasional collaboration with Barry Barnes and Steven Shapin, make "controversy" the central metaphor in the history of biometry.[31]

The most complete published account is Donald MacKenzie's book-length study of Statistics in Britain, 1865-1930. MacKenzie's account situates the history of biometry in the context of the development, between 1865 and 1930, of statistical theory as a scientific specialty. The central characters of MacKenzie's narrative are Francis Galton, Karl Pearson, and R.A. Fisher. In MacKenzie's story, each character represents a specific period of that development.

Galton, "Victorian gentleman scientist, explorer, [and] pioneer," represents the period from the late

1860s to the late 1880s, in which Galton worked out the basic concepts of regression and correlation, thereby clearing the way for the application of statistical analysis to problems involving more than one variable.[32]

Pearson dominates the second period in the development of statistical theory, from the mid-1890s to the First World War, in which he assembled a coherent group of researchers, built the first university department principally concerned with statistical theory, taught the first advanced courses in statistical theory in Britain, and established and edited Biometrika--the biometricians' major vehicle for publication. During this period, Pearson developed and systematized Galton's insights, and extended them by developing "the standard formula for the correlation coefficient and the widely used 'chi-square' test of the goodness of fit between observations and theoretical predictions."[33]

Fisher dominates the last period of MacKenzie's narrative, the period following the First World War and lasting into the early 1930s. MacKenzie's Fisher "pioneered a new role for the statistician--that of active involvement in agricultural and biological experiments."[34]

Concerned to demonstrate a connection between statistical theory and British society, MacKenzie's narrative focuses on the fact that all three of his major characters were eugenists. Eugenics is the name given to various related attempts to apply a scientific understanding of inheritance and evolution to the improvement of the hereditary qualities of a race. In Britain, the core of the eugenics movement was the Eugenics Education Society founded in 1907. In MacKenzie's analysis, eugenics was, and is, an ideology expressing particular social interests, specifically those of a rising professional middle class. In demonstrating that eugenics affected the content of the statistical work of Galton, Pearson, and Fisher, MacKenzie illustrates his point that the content of science is affected by external sociological factors.[35] To accomplish this goal in his narrative, MacKenzie presents a tale of two controversies.

The first of the controversies pitted biometrician versus Mendelian. In contrast to Provine's characterization of the controversy as a product of the conflict between two views of evolution needlessly exacerbated by strong personalities, MacKenzie fashions a story of a battle between two fundamentally different and "incommensurable" approaches to the problem of heredity.[36] In that context, the combatants'

commitment to different views of evolution is not an explanation, but something to be explained. The explanation is to be found by identifying the goals, objectives, and social interests of the two competing groups.[37]

In MacKenzie's analysis, the Mendelian approach to the problem of heredity was shaped by the belief

> that the prime aim of the science of heredity should be the development of a theoretical model of the process of heredity--the development of an account of the passage from parent to offspring of the factors that determined the observable characteristics of organisms, of the "genotypes" that led to observable "phenotypes."[38]

In contrast, the biometricians were primarily concerned with detailing and measuring the resemblances of "phenotypes."

The incommensurability of these differing aims, however, was surmountable. Each of the combatants was capable of understanding and using each other's concepts and often used them in the process of the debate. Therefore, the incommensurability of the two approaches was also something that required an explanation. In search of such an explanation, MacKenzie turns to the "different sorts of skills employed by the two sides"--mathematical and biological.[39] But while differing training and skill acquisition was clearly important, there were also instances of individual action in the face of that training. For example, both Weldon and

Bateson repudiated the morphological and embryological training they received from the their Cambridge mentors. MacKenzie reconciles this apparent paradox by pointing out that science is inherently competitive.

> Prestige and reward follow in part from the recognition, by their fellows, of scientists' work as correct and interesting. In this "market" the scientists' resources include the skills relevant to the performance of successful scientific work that they possess....Thus we can expect there to arise a tendency to evaluate new theoretical developments, new techniques, and so on, in terms of their effect on the value of scientists' existing skills.[40]

By identifying scientific activity with the metaphor of the marketplace, MacKenzie produces an understanding of the biometry-Mendelism controversy in which each side can be seen to have been making scientific judgments that were influenced by their need to sell their own expertise. Finally, the biometricians' goals exhibited a "formal parallelism" with the goals of eugenics. Success to the biometricians meant the development of techniques that allowed for the "prediction of the effects of intervention in one generation on the measurable characteristics of subsequent generations."[41] Such knowledge was precisely what was required to guarantee the success of a program of eugenic intervention.

Seen in this context, the biometricians' view of evolution as a gradual and continuous process, which

they were almost alone in defending in 1900, becomes a view that was

> peculiarly appropriate to the job of justifying gradual, planned social change; . . . a key legitimation of the social role of the intellectual and scientific expert; . . . and a channel by which social interests--those of the professional middle-class expert--were brought to bear on the construction of biological knowledge.[42]

The opposition of the Mendelians, represented in MacKenzie's narrative by William Bateson, is also understood in terms of appropriate social interests. Specifically, MacKenzie argues that Batesonian biology carried a social message appropriate to the traditional academic elite with which Bateson had come to identify. Building upon earlier work done by William Coleman, MacKenzie characterizes Bateson's thought as "romantic-conservative," in the sense that it reflected an opposition to the values of bourgeois society from the standpoint of an idealized past.[43] In short, MacKenzie's narrative understands the biometry-Mendelism controversy in terms of an ideological opposition between the rationalist individualism that characterized British utilitarianism and the romantic-conservativism of traditionalists.

MacKenzie builds his case by showing that anti-utilitarianism was an integral part of Bateson's world view. Bateson had criticized orthodox Darwinism as "a utilitarian view of building up species."[44] At the

same time that he was developing his opposition to orthodox Darwinism, Bateson was also leading the opposition to the abolition of compulsory entrance qualification in Classical Greek, defending the notion of a "Classical System" of education against mere "Technical Education."[45]

Where the question of eugenics was concerned Bateson is seen as a man "torn"; realizing that "eugenic measures might well be in the interests of the intellectual and professional class to which he belonged," but fearing that "their success might merely continue the process of the encroachment of the utilitarian rationalization and modernization against which he had set himself."[46]

MacKenzie concludes that there were patterns in the disagreement between the biometricians and the Mendelians.

> On the one hand, we find in the writings of the biometric school the view of nature as orderly, predictable and in gradual mass progress alongside the advocacy of orderly, predictable and gradual collective social change, particularly through the eugenic improvement of the innate characteristics of entire human populations. On the other hand, in the writings of Bateson we find the view of biological evolution as the result of the sporadic appearance of qualitatively different varieties alongside the claim that all that is socially worthwhile springs from the unpredictable appearance of genius; the view that the organism is holistically ordered alongside the view that society ought to be similarly ordered; even the view of evolution as loss alongside the condemnation of what the conventional called progress.[47]

The patterns are traces of the effects of external social factors impinging on the internal development of science.

The second controversy in MacKenzie's narrative occurred within the context of the development of statistical theory. It concerned differing approaches to the measurement of association between nominal variables. Francis Galton had provided the necessary concepts--regression and correlation--for dealing with interval variables, or those variables, such as height and weight, for which a measurement scale with a valid unit of measurement existed. By 1900, British statisticians like F.Y. Edgeworth and Karl Pearson had refined and extended these concepts into a working method for dealing with interval-level variables. Thereafter, nominal variables, or those variables for which no valid unit of measurement was available, became the focus of statistical theoreticians.[48] MacKenzie provides an account of what he considers to be the two main attempts to develop a theory of association of nominal variables, one by Karl Pearson and one by George Udny Yule.

Beginning with data presented in a two-by-two or contingency table, Yule's theory of association of nominal variables, which MacKenzie characterized as "extremely direct," produced a coefficient of

association (designated as Q) which satisfied three conditions:

> Firstly, it should it should be zero if and only if A and B are non-associated or independent....The second property is that the coefficient should be +1 when, and only when, A and B are completely associated....The third property is that the coefficient should be -1 when A and B are completely associated in a negative sense.[49]

Pearson produced a coefficient of association that he termed "r" by what MacKenzie characterizes as "a much tighter but more precarious theoretical argument," which assumed that the observed categories that constituted the nominal variable contingency tables corresponded to ranges of more basic interval variables whose distribution was bivariate normal.[50] In other words, Pearson worked from the assumption that the existence of a relationship between apparently discrete phenomena was always evidence for the existence of gradual and continuous variation distributed along a normal curve. Accordingly, Pearson's "r" which could serve as a coefficient of association for nominal variables, was derived from the correlation of the assumed underlying variables, or the "tetrachoric coefficient of correlation."

A controversy over which method was best raged publicly from 1905 to 1913 and remained unresolved, as far as the combatants were concerned, even at Pearson's death in 1936.[51] In the course of the controversy,

Yule attacked the assumption behind Pearson's method, arguing that there were many instances in which the application of a coefficient of association based on an assumption of underlying continuous variables was patently absurd. For example, Yule pointed to the use of Pearson's method on vaccination statistics and remarked that "all those who have died of small-pox are equally dead...and the dead are quite distinct from the survivors."[52]

Pearson and his associates, on the other hand, accused Yule of reifying his categories. "The controversy between us," wrote Pearson and David Heron, "is the old controversy of nominalism and realism. Mr. Yule is juggling with class-names as if they represented real entities, and his statistics are only a form of symbolic logic."[53] The biometricians defended their assumption of underlying continuous variation with an argument about its utility.

> The coefficient of correlation has such valuable and definite physical meanings that if it can be obtained for any material, even approximately, it is worth immensely more than any arbitrary coefficients of "association" and "colligation."[54]

The appeal to utility in Pearson's defense of his method provides MacKenzie with an opening for an analysis of the controversy over the contingency table that parallels the one provided for the biometry-Mendelism controversy.

Pearson's goal was to maximize the analogy between interval and nominal variables, in order to transfer the predictive reliability of interval level statistical analysis to nominal level analysis. In MacKenzie's analysis, the maintenance of the interval/nominal analogy that structured Pearson's approach was necessitated by Pearson's objective to construct "a mathematical theory of descent, in order to be able to predict from the knowledge of an individual's ancestry the characteristics of that individual."[55] That objective was in turn necessitated by the requirements of the eugenic program to move "from prediction to potential control over evolutionary processes."[56]

Yule's goal was to create a general theory of association. For him, the interval/nominal analogy offered no particular advantage. Yule had no commitment to eugenics. Rather, as MacKenzie shows with reference to Yule's private correspondence, Yule's attitude towards eugenics was a mixture of indifference and hostility.[57]

The final piece of evidence in MacKenzie's argument is numerical. He shows that of the small group of scientists who contributed to the development of statistical theory in Britain from 1900 to 1914 (a group of 26 individuals), 12 of them could be considered as members of the biometric school and 10 of them were

attached in some way to Pearson's approach. The other 14 individuals "had a wide variety of affiliations, and included civil servants, administrators and one industrial scientist, as well as university staff."[58] Of these, only one was found to have been enthusiastic about Pearson's approach. Thus, Pearson's approach, and the statistics that it produced, "was a social institution that can be explained in terms of the connection between biometric statistics and eugenics."[59] As such, it was a social institution "ultimately sustained by professional middle-class interests."[60]

The more general and diffuse goals that structured Yule's approach reflected "a focus on administration" that was characteristic of the concerns of the Fellows of the Royal Statistical Society--an organization in which Yule was an active member.[61] Finally, MacKenzie concludes that "it is just possible" that the absence of the eugenics/statistics connection might reflect the operation of the interests of a "traditionalist opposition to eugenics" at work in Yule's approach.[62]

The controversy over the contingency table was never resolved. Instead, as with the biometricians' debate with the Mendelians, the disagreement became obsolete as a new approach to eugenics and statistics was developed most notably in the work of R.A.

Fisher.[63] As MacKenzie's tale moves on to describe Fisher's new approach and the social interests that it reflected, Karl Pearson and his biometric school, their role played out, fade from the narrative.

## Historical Writing and Identity

The two story lines that constitute the history of biometry as we know it are very different. Each fashions its subjects and objects in different ways. In the first story, the subject is the history of the development of theoretical population genetics and the modern synthesis. In the narrative, identities are fashioned for both the researcher and the objects being researched. The author is cast as both scientist and historian, narrating a tale that explicates the historically contingent, but inevitably progressive, process of discovery. The biometricians, as objects of historical inquiry, are scientific pioneers--individuals characterized by unusually strong personalities--on a journey of discovery. Along the way, the biometricians take right and wrong turns. The right turns are the result of the kind of clear reasoning and insightful innovation that would lead, eventually, to the development of modern evolutionary theory. The wrong turns--detours and historical paradoxes--are the result of the clouding tendencies of personal emotions.

In the revisionist story, the subject is the sociology of scientific knowledge. The author appears in the role of social scientist. Leading the reader through an investigation of a case study, he presents a hypothesis--that scientific judgments are affected by social interests in specific ways--and tests that hypothesis by examining the curious case of the biometricians and their twin controversies. The biometricians, as objects of sociological and historical investigation, are a group, pursuing goals and objectives that reflect the social position of the group. The demonstrated effect of the pursuit of those goals on the content of biometric science negates the assertion, implicit in the first story, of the logical progression of science.

For all their differences, however, the two stories have much in common. They are meticulously researched, copiously documented, and persuasively written. In short, they are both examples of first-rate history, and we may easily imagine that both stories reflect an understanding of the past actuality of the history of biometry. That they tell different tales about different historical objects simply serves as a reminder that our historical understanding of the past is fictive.

## Notes

1. William B. Provine, The Origins of Theoretical Population Genetics (Chicago/London: University of Chicago Press, 1971).

2. Provine, Origins, p. ix. Provine's Origins led to a lively and productive debate, chiefly between Provine and Ernst Mayr, over who and what was really most important in bringing about the modern synthesis. See Ernst Mayr, "Essay Review: The Recent Historiography of Genetics," Journal of the History of Biology 6 (1973): 125-155, and Ernst Mayr and William B. Provine, eds., The Evolutionary Synthesis: Perspectives in the Unification of Biology (Cambridge, Ma.: Harvard University Press, 1980). Recently, it has been persuasively argued that the modern synthesis, when seen in the larger context of a movement to unify all sciences exemplified by the work of the Vienna Circle logical positivists, signals the final unification of the biological sciences. See V.B. Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," Journal of the History of Biology 25 (1992): 1-65.

3. Provine, Origins, pp. 12-13.

4. Ibid., pp. 14-23. A standard biographical study of Galton's life and work is Derek W. Forrest, Francis Galton: The Life and Work of a Victorian Genius (New York: Elek, 1974).

5. Francis Galton, Natural Inheritance (London: Macmillan, 1889), p. 104. Quoted in Provine, Origins, p. 21.

6. Galton, Natural Inheritance, p. 136. Quoted in Provine, Origins, p. 22.

7. Provine, Origins, p. 23.

8. The swamping argument was brought to bear most forcefully as a contemporary criticism of Darwin's theory in Fleeming Jenkin, "On the Origin of Species," North British Review 46 (1867): 277-318. For a full account of the significance of the swamping argument in the development of evolutionary theory, see Peter Vorzimmer, "Charles Darwin and Blending Inheritance," Isis 54 (1963): 371-390.

9. Francis Galton, Finger Prints (London: Macmillan, 1892), p. 20.

10. Provine, Origins, p. 25.

11. For an early descriptive account of the controversy, see P. Frogatt and N.C. Nevin, "The Law of Ancestral Heredity and the Mendelian-Ancestrian Controversy in England, 1889-1906," Journal of Medical Genetics 8 (1971): 1-36. For Mendelism, see Robert Olby, Origins of Mendelism, 2nd ed. (Chicago: University of Chicago Press ,1985).

12. Provine, Origins, pp. 27-28.

13. Karl Pearson, Speeches Delivered at a Dinner Held in University College, London, in Honour of Professor Karl Pearson, 23 April 1934 (Cambridge: Cambridge University Press, 1934), pp. 22-23.

14. Provine, Origins, p. 29. Biographical work on Pearson and Weldon is surprisingly slim. For Pearson, there is a tribute from his son, Egon Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," Part I, Biometrika 28 (1936): 193-257; Part II, Biometrika 29 (1938): 161-248; and Churchill Eisenhart, "Karl Pearson," Dictionary of Scientific Biography 10 (1970-1980): 447-473. For Weldon, there is Karl Pearson, "Walter Frank Raphael Weldon," Biometrika 5 (1906): 1-52; and Ruth Schwartz-Cowan "Walter Frank Raphael Weldon," Dictionary of Scientific Biography 14 (1970-1980): 251-252.

15. Provine, Origins, p. 29.

16. Ibid., p. 31.

17. Ibid., p. 35.

18. Recorded by R.C. Punnett, "Early Days of Genetics," Heredity 4 (1950): 2. Quoted in Provine, Origins, p. 37.

19. For Bateson and his contributions to Mendelism, see Olby, Origins of Mendelism; William Coleman, "Bateson and Chromosomes: Conservative Thought in Science," Centaurus 15 (1970): 228-314; and Lindley Darden, "William Bateson and the Promise of Mendelism," Journal of the History of Biology 10 (1977): 87-107.

20. William Bateson, "On the Variations in Floral Symmetry of Certain Plants Having Irregular Corollas," Journal of the Linnean Society (Bot.) 28 (1891).

21. Provine, Origins, pp. 43-44.

22. W.F.R. Weldon to William Bateson, 15 February 1894, No. 13, Bateson Papers, Baltimore. Quoted in Provine, Origins, p. 44.

23. Provine, Origins, p. 45.

24. Ibid., pp. 57-58.

25. Ibid., pp. 108-129.

26. Ibid., pp. 131-140.

27. Ibid., pp. 140-154.

28. Ibid., pp. 167-177.

29. Ibid., pp. 154-167.

30. Ibid., p. 25.

31. The works that comprise the published results of this approach to the history of biometry include Donald MacKenzie, The Development of Statistical Theory in Britain, 1865-1925: A Historical and Sociological Perspective. Ph. D. Dissertation, University of Edinburgh, 1977; Donald MacKenzie. "Eugenics in Britain," Social Studies of Science 6 (1976): 499-532; Donald MacKenzie and Barry Barnes, "Scientific Judgment: The Biometry-Mendelism Controversy," pp. 191-210, Natural Order: Historical Studies of Scientific Culture, Barry Barnes and Steve Shapin eds., (California: Sage Press, 1979); Donald MacKenzie, "Statistical Theory and Social Interests: A Case Study," Social Studies of Science 8 (1978): 35-83; Donald MacKenzie, Statistics in Britain, 1865-1930: The Social Construction of Knowledge (Edinburgh: Edinburgh University Press, 1981).

32. MacKenzie, Statistics in Britain, pp. 9-10. I will treat the concepts of regression and correlation, as well as other important statistical innovations in Chapter 5.

33. Ibid., p. 10.

34. Ibid.

35. Ibid., p. 12. For the connection between the work of Francis Galton and eugenics, MacKenzie drew upon a series of articles by Ruth Schwartz-Cowan: Sir Francis Galton and the Study of Heredity in the Nineteenth Century, (Doc. Diss.) Ann Arbor, 1969; "Francis Galton's

Contribution to Genetics," Journal of the History of Biology 5 (1972): 389-412; "Francis Galton's Statistical Ideas: The Influence of Eugenics," Isis 63 (1972): 509-528; and "Nature and Nurture: The Interplay of Biology and Politics in the Work of Francis Galton," Studies in the History of Biology 1 (1977): 133-207. For connections between the eugenics movement and Karl Pearson and his biometric school, and for the notion of scientific controversy as a case study, MacKenzie utilized the work of Lyndsay A. Farrall: The Origins and Growth of the English Eugenics Movement, 1865-1912 (Doc. Diss.) Ann Arbor, 1970; "Controversy and Conflict in Science: A Case Study--the English Biometric School and Mendel's Laws," Social Studies of Science 5 (1975): 269-301. Subsequently, Farrall has also published "The History of Eugenics a Bibliographical Review," Annals of Science 36 (1979): 111-123.

36. Here MacKenzie draws on Paul K. Feyerabend, "Explanation, Reductionism, and Empiricism," pp. 28-97, in H. Feigl and G. Maxwell (eds.), Scientific Explanation, Space and Time (Minnesota Studies in the Philosophy of Science 3) (Minneapolis: University of Minnesota Press, 1962); and T.S. Kuhn, The Structure of Scientific Revolutions (Chicago: Chicago University Press, 1970).

37. More specifically, the approach taken by the Strong Programme seeks to explore "the relationship between structures of the scientific community and society at large...and society and its structures to particular people." Towards this end, the researcher examines the historical record attempting to identify "theoretically plausible relationships between belief and social position while accepting that these relationships may in actuality be hidden or only partially manifest." The procedure involves identifying "social positions whose occupants may reasonably be held to have similar interests and experiences. We may then argue that these interests and experiences constrain the set of beliefs `appropriate' to occupants of these positions." Appropriate beliefs are defined as "ones justifying a group's privileges, advocating an advance in its situation, furthering its coherence or the interests of its members, and reflecting the social features of the typical experience of its members." MacKenzie, Statistics in Britain, pp. 4-5.

38. MacKenzie, Statistics in Britain, p. 122. The quotation marks around the terms genotype and phenotype contribute to the sense of hindsight that the story

creates. They let the reader know that these are not terms that the biometricians and Mendelians of the late-nineteenth century would have used. But they are the modern way of describing what those nineteenth century scientists were up to.

39. This approach had been suggested by Robert de Marrais, "The Double-Edged Effect of Sir Francis Galton: A Search for the Motives in the Biometric-Mendelian Debate," Journal of the History of Biology 7 (1974): 141-174; and by Garland E. Allen, "Genetics, Eugenics, and Class Struggle," Genetics 79 (1975): 29-45; and in Allen's "Genetics, Eugenics, and Society: Internalists and Externalists in Contemporary History of Science," Social Studies of Science 6 (1976): 105-122.

40. MacKenzie, Statistics in Britain, p. 127.

41. Ibid., p. 137.

42. Ibid., pp. 137-138. MacKenzie further notes that the "chronology--that Pearson's political use of the idea preceded his biological defense of it--effectively disposes of the notion that the course of events was of the later discovery of the social implications of biological theory." p. 138.

43. Ibid., p. 143. See also William Coleman, "Bateson and Chromosomes: Conservative Thought in Science," Centaurus 15 (1970): 228-314. Coleman characterized Bateson simply as a "conservative," following the definition provided in Karl Mannheim, Essays in Sociology and Social Psychology (London: Routledge and Keegan Paul, 1953). MacKenzie prefers Mannheim's "romantic-conservative" category. While Mannheim's original work concerned romantic-conservativism in Germany as it defined itself in opposition to the ideologies associated with the French Revolution, MacKenzie suggests that analogues within British history are easy to find. For example, the "culture and society" tradition identified by R. Williams, Culture and Society, 1780-1850 (Harmondsworth: Penguin, 1968); and the upholders of the "aristocratic ideal" identified by H. Perkin, The Origins of Modern English Society, 1780-1880 (London: Routledge and Keegan Paul, 1972).

44. William Bateson, Materials for the Study of Variation (London: MacMillan, 1894), p. 11. Quoted in MacKenzie, Statistics in Britain, p. 145.

45. MacKenzie, Statistics in Britain, p. 145.

46. Ibid., p. 147.

47. Ibid., p. 151.

48. Ibid., p. 153. See also Chapter 5 below.

49. MacKenzie, Statistics in Britain, pp. 154-155.

50. Ibid., pp. 156-157.

51. The obituary that Yule wrote at the time of Pearson's death stated that only time would tell who was right.

52. G.U. Yule, "On the Methods of Measuring Association between Two Attributes," Journal of the Royal Statistical Society 75 (1912): 139-140. Quoted in MacKenzie, Statistics in Britain, p. 162.

53. Karl Pearson and David Heron, "On Theories of Association," Biometrika 9 (1913): 159-315. p. 302. Quoted in MacKenzie, Statistics in Britain, p. 164.

54. Pearson and Heron, "On Theories of Association," p. 300.

55. MacKenzie, Statistics in Britain, pp. 168-169.

56. Ibid., p. 169.

57. Ibid., p. 173. Specifically MacKenzie cites correspondence between Yule and Major Greenwood from 1912 to 1920.

58. MacKenzie, Statistics in Britain, pp. 175-176.

59. Ibid., p. 177.

60. Ibid., p. 180.

61. Ibid., p. 174.

62. Ibid., p. 180.

63. Ibid., p. 179. MacKenzie suggests that, in contemporary statistical opinion, a "theoretical pluralism" reminiscent of Yule's approach seems to have won out. However, he also notes that "one aspect of Pearson's approach--the construction of models to fit the data" has become increasingly important in contemporary statistics. p. 180.

# CHAPTER 2
# A HISTORY OF ITS OWN?

Chapter 1 examined the two works most responsible for our current historical understanding of the science of biometry. The first was a history that identified the biometricians as characters in a story about the development of modern evolutionary theory. The second was a more sociologically oriented history in which the biometricians became an example of the ways in which social interests and influences could affect the goal orientation and, therefore, the content of scientific theories. The conclusion of Chapter 1 was that our entire historical understanding of biometry is constituted within the narrative strategies of two works which were primarily concerned with present-day issues larger than biometry.

That conclusion raises the issue of whether or not it is desirable, or even possible, to write a history of biometry "for its own sake." Is it desirable or possible to subvert the narratives that have fashioned our understanding of biometry and to interrogate the historical record without concern for contemporary issues? Can we find the authentic biometry? To answer

that question, this chapter returns to the historical record, to the "primary sources," and pursues three strands of narrative that are ignored or under-developed in the historiography: the story of the research institute that Karl Pearson built and tried to maintain, the story of the publication and dissemination of Biometrika, and the story of the people within the institution and their work.

## The Research Institute

On the occasion of the presentation of a marble bust rendered for him by his friend, the sculptor H.R. Hope Pinker, Karl Pearson was engaged in one of his more passionate vocations--the issuing of a gloomy warning about the future. On that particular night, Pearson was concerned with the future state of the research institute that he had forged out of the merger of the Biometric and Galton Laboratories housed in University College London's Department of Applied Statistics. "I can conceive," Pearson warned his audience, of a time when the department, "under a director with different tastes and fewer bristles than myself," would be visited by

> a professor of anatomy raiding this laboratory for its cranial collections; a professor of archeology considering the contents of its museum better placed in his own department; a professor of zoology believing its animal house ought essentially to be part of his domain; and professors of mathematics and medical history

> raiding our library for the sake of its journals and source books of tables. We might have an engineer or an applied mathematician peeking beneath the dust wrappers and marching off with our integrators and analysers.[1]

As an afterthought, perhaps remembering the occasion, Pearson added that a fine-arts professor might come and carry away the Pinker bust, not because of any lasting memory of its subject, but simply for its value as an example of a great artist's work.[2]

Pearson had good reason to fear for his creation. It was an unprecedented collection of pursuits which reflected Pearson's singular vision, but not the ordinary lines of academic demarcation. Furthermore, its relationship to University College and to the University of London was ambiguous. The cornerstone in this unlikely structure was the Biometric Laboratory. Pearson always located the origin of the biometric laboratory in the year 1895, when he (as Professor of Applied Mathematics and Mechanics at University College) gave his first lecture on "the mathematical theory of statistics."[3] From these lectures, he gathered around him a small group. Working out of a single room at University College, the group began to issue papers, mostly under the auspices of the Royal Society, which, in Pearson's view, constituted the foundation of what would later come to be known as the "English School" of mathematical statistics.

Pearson's collaboration and budding friendship with W.F.R. Weldon led him to apply his statistical expertise to biological questions. Pearson and Weldon became close friends during Weldon's tenure as Jodrell Professor of Zoology at University College from 1890-1899, and remained so after Weldon's appointment as Linacre Professor of Comparative Anatomy at Oxford in 1899. In 1901 the founded the journal Biometrika.

In the summer of 1903, the Worshipful Company of Drapers, one of London's oldest chartered companies, made a grant to the University of 1000 pounds for the support of "higher learning." The money was funnelled to Pearson for the operation of his Biometric Laboratory. In 1905, a grant of 2000 pounds was issued to continue support of the laboratory for an additional five year period. This grant was renewed every four or five years until 1932.[4]

Meanwhile, Francis Galton had donated 1500 pounds to the University of London to further the study of eugenics. The grant led to the establishment of the Eugenics Records Office housed initially at 50, and later at 88 Gower Street. Towards the end of 1906, Galton, then 84 years old, persuaded his friend Pearson to assume control of the Eugenics Office which was then re-named the Francis Galton National Eugenics Laboratory. The Galton Laboratory was moved to rooms at

University College as soon as space became available in 1907.[5]

Upon Galton's death in 1911, the remainder of his estate was bequeathed to the university to establish the Galton Chair of Eugenics. The Galton Professorship was awarded to Pearson, thereby relieving him of the arduous teaching duties of the Professor of Applied Mathematics. The Biometric and Galton laboratories were effectively merged under the direction of Pearson who became head of the newly created Department of Applied Statistics. From this position Pearson directed the activities of the joint laboratories, housed in four small rooms at University College, and pursued his vision of a research institute for the creation and propagation of a new kind of science.[6]

Lack of funds, insufficient housing, and inadequate supply and publication budgets hindered his efforts. However, in 1912 an anonymous donor (later revealed to be Sir Herbert Bartlett) responded to one of Pearson's numerous public fund raising appeals by offering to cover the cost of a building to house the combined laboratories. The building was constructed on the Gower street frontage of University College as an addition to the construction of the School of Architecture that was already underway.[7] Pearson had reservations about the site. He wrote Weldon's widow that it was "not indeed

an ideal situation; i.e., on the frontage which is noisy and not the best for breeding work of any kind."[8] But the location did not dampen his enthusiasm as the dream of his research institute seemed at last achievable:

> Once get this and we can go forward to other things I dream of!
>
> I want to see a double staff with a zoologist and a medical officer and a biometric farm such as we planned in the good old days![9]

His only real laments were that Weldon had not lived to see the dream realized (having died in 1906) and that it had come so late in his own life: "How he [Weldon] and I could have worked it out together, if the fates had been on our side! An now one is growing too old!"[10]

The plans for outfitting the building were elaborate and reflected the idiosyncracies of Pearson's vision. The ground floor was to be the "public floor," and was to house "a large museum which will illustrate heredity, statistical processes, and social problems," a lecture theatre, and rooms for committee meetings, exhibitions and publication offices.[11] The museum was to be used as a "lure" to attract visitors who could then be enticed into the "anthropometric laboratory," also planned for the public floor, where they would be poked, prodded and meticulously measured, forced to yield "data" for future study.

The first floor was to house a library, classrooms, staffrooms, and two laboratories, one for photographers

and the other for junior workers, while the basement would hide cloak and service rooms and provide storage space for the large craniological and osteometric (long bone) collections. The second floor was to serve as the main research floor. Plans called for

> a photographic studio with a darkroom, a large room for biometric workers in craniometry, a workshop, a room for special experimental work, and two spacious rooms, the one for archives (stored observations, pedigrees and schedules) and the other for the instruments (integrators, analysers, and curve plotters) and for the use of draughtsmen.[12]

The building itself was completed in 1914 and was supposed to be ready for occupancy around Christmas time of that year. At that moment, Pearson stood (finally, at the age of 57) on the brink of realizing his dream of a research institute devoted to the new science of biometry.

The outbreak of World War I shattered the moment, as the building was pressed into service as additional hospital space for wounded soldiers. Pearson and the staff, still in their old rooms, offered their calculating skills to the service of the war effort--partly out of a sense of duty and partly as a strategy on Pearson's part to keep his highly trained group together. At first the group was put to work for the Board of Trade's Labour Department, analyzing national unemployment data. When unemployment ceased to be a problem, they aided the Census Production Department by

charting the seasonal demand for raw materials whose importation had become difficult.[13]

Pearson's strategy did not ultimately succeed. His best people were constantly being siphoned off by various governmental departments who needed their expertise, and who could pay more than the subsistence wages than Pearson's grants could muster. By 1916 Pearson felt that all of his energies were going into the constant training of new people. He was also having to simultaneously re-tool himself and the staff to handle the new, more directly war-related work that was coming their way--analyzing strains on airplane propellers and plotting bomb trajectories. In 1918 an exhausted Pearson requested that the direction and coordination of the group's efforts be taken over directly by the Ministry of Munitions. For the final summer of the war, Pearson was almost alone in the laboratory's rooms, until finally retiring for a month's holiday in the country.[14]

Recovery from the ravages of war was a slow and painful process for all of Britain, and Pearson's laboratories were no exception. Most of his staff had been lost and he lacked the funds to attract competent replacements. Pearson, and such staff as he still employed, managed to occupy the first floor of the Bartlett Building in October of 1919, but the future

outlook was bleak. Pearson produced and distributed a "History of the Biometric and Galton Laboratories," and wrote an open letter to the Times in an effort to focus public attention on the present and future value of the laboratories' services. "The war has crippled institutions as well as men," wrote Pearson, "and unless a fund is forthcoming for the present endowment of the joint laboratories, they will be left in worse financial position than in 1914."[15] An official opening of the Bartlett building, presided over by the Minister of Health, was held in June of 1920. The emphasis of the evenings festivities was on fund-raising. In his speech that night, a portion of which was reprinted in the 17 June edition of Nature, Pearson reminded his audience that "in 1871, the German nation made the extension of old, and the founding of new, universities a first claim on their war endemnities." In contrast, Pearson lamented the fact that "in 1920, we hear no suggestion that from our universities a new national life has to spring."[16]

In his request for the extension of the Drapers' Company Grant in April of 1921, Pearson could report that ten post-graduate students now worked in the laboratories, and that the numbers of undergraduates being taught was expected to increase. Also, the London City Council supplied funds for a medical officer to work with Pearson and his staff. Still, Pearson

concluded that the existence of the laboratories was in a "precarious position."[17]

However, his next report to the Drapers' Company in 1924 reveals that a degree of prosperity had gradually returned. Between 1922 and 1924, a donation from long-time staff member Ethel Elderton made it possible to open the Anthropometric Laboratory and its "lure," the museum, which now boasted a collection "illustrating the inheritance of good and bad qualities in man," and an exhibit of the "history of early man and his artifacts for the past 200,000 years."[18] The "animal house" was also opened in 1922, although it appears to have been a mixed blessing as Pearson reports that "the charge of dogs and mice has involved new and somewhat onerous duties on members of the staff."[19]

The period from the mid-1920s to 1932, when Pearson decided to retire from all duties except the editing of Biometrika, was a time of relative stability and steady output from the joint laboratories. There were still problems. Funding was not as secure as Pearson would have liked (it was never on a par with research institutes in America and Germany), and staff turnover continued due to the lack of permanent positions. Nevertheless, the number of staff remained steady at ten and Biometrika was once again self-supporting. Pearson

even felt secure enough to launch a second publication, The Annals of Eugenics, in 1925.

However, there were also good reasons for Pearson's fear that his research institute would one day be uprooted. It was something of a hot-house flower in the ever-changing environment of the British academic landscape. While housed in Pearson's Department of Applied Statistics, the two laboratories that constituted Pearson's institute were in different administrative situations. The Galton Laboratory, and its attending Galton Professorship, were funded and governed by the stipulations of the Galton bequest which had been made to the University of London. The Biometric Laboratory was funded by the Drapers' Grant and was related, in a somewhat ambiguous way, to University College.

As he campaigned for funds and justified the institute's existence, Pearson emphasized advanced research and the training of future researchers. Graduate students, not undergraduates, formed the educational mission that he envisioned. Accordingly, Pearson argued that the joint laboratories which formed the institute should not be judged, as many other departments were, by the number of undergraduates they served. He emphasized the time and effort that post-

graduate training required, and lobbied for a larger staff to accomplish the task.

Playing the academic game however, Pearson could speak almost wistfully of teaching undergraduates. When critics called him to task for ignoring this segment of the student population, he reiterated his request for the College to sanction an undergraduate exam in applied statistics. Underneath the rhetoric, however, was a determination to continue the research mission that he had set for himself and his staff. After the war, when an increased number of undergraduates requested study with his faculty, he resisted the requests.

Nevertheless, the demand to do more undergraduate teaching persisted. In some ways, Pearson was a victim of his own successful campaign. Decades of effort to portray applied statistics as the expertise necessary for future success in both science and business, together with some successful placement of former pupils into positions of influence in both fields, had generated a demand for undergraduate instruction. At University College in particular, many felt that this demand should be met through the establishment of a Department of Statistics. Pearson thought that a new Department of Statistics whose main mission would be to instruct undergraduates was a fine idea, but he did not

want it created out of the ransacked ruins of his beloved research institute.

While Pearson fought the encroachment of education on his research mission, the Galton Laboratory's traditional approach to eugenics was coming under increasing criticism. Pearson's high-profile opposition to Mendelian genetics was increasingly perceived as dogmatic and old-fashioned. While Pearson remained at the head of his department, his research institute was safe. His critics could cry from the sidelines, but no one dared to oppose the man whose reputation for contributions to the development of statistical theory was second only to his reputation as a tenacious and furious intellectual warrior. As time for his retirement drew near, however, it became obvious that he would outlive his dream.

The tone of his speech at the 13th annual Galton Dinner in January of 1932 was bitter:

> From now onward it is fitting that the guardianship, which my friend [Galton] entrusted to me, should cease, and the Laboratory develop along those newer lines, which I am told have effectively replaced the old fashioned ideas of Galton and his school.[20]

The rest of the speech was devoted to a passionate defense of those ideas.

Pearson's nightmarish vision of the plundering of his research institute sprang from the very real danger that two forces, the growing demand for undergraduate

instruction in statistics and the call for a more "modern" (that is, genetic) approach to eugenics, would, when he was no longer there to resist, rip his fragile creation apart. His fear was well founded and his vision was prophetic.

In 1932, Pearson gave the College a year's notice of his impending retirement. At first Pearson was hopeful that the spirit of his institute would be preserved. He approved of the committee the College appointed to consider the future of his department and of the Galton and Biometric Laboratories. In November of 1932 the Provost of the College accepted one of Pearson's suggestions for the committee and expressed the belief that Pearson's thoughts seemed to be "moving very much on the same lines as those of the College."[21] However, when a draft of the committee's proposal reached Pearson six days later he learned that his domain was to be split in two. The committee's recommendation was to transform the Galton Laboratory into a Department of Eugenics. The new department would be home to the old institute's anthropometric work, and Pearson's successor as Galton Professor would direct it. They further recommended that the rest of the Biometric Laboratory be subsumed under a new Department of Statistics and directed by a newly-appointed Reader or Professor of Statistics.[22]

Pearson fought desperately to save his dream. He fired off letters to the provost stressing that the laboratories constituted a single research institute, and to the principal calling the committee's plan a breach of trust and outlining what he saw as its disastrous consequences: "No one will give anything further to an institute for a definite research purpose when they know that their gifts may be used by undergraduates for quite different purposes."[23] When these efforts failed, Pearson appealed to a higher authority, writing to Lord Macmillan at the University Court. Lord Macmillan could see no legal problem with the committee's plan, however, and Pearson was faced with a <u>fait accompli</u>.[24]

One colleague, who had some knowledge of the inner workings of the committee, tried to console Pearson by telling him that "the long and short" of the committee's decision was what they considered to be the "impossibility of finding, or hoping to find, a second K.P."[25] Although Pearson was forced to accept the inevitable, and graciously and gratefully accepted tributes offered him in the form of retirement dinners, "K.P." was never entirely consoled. Even the appointment of his twin successors was a bitter-sweet legacy: his son Egon Sharpe Pearson was appointed head of the new Department of Statistics, while R.A. Fisher,

the leading figure of the genetic approach to eugenics, became the second Galton Professor.

## Biometrika

Karl Pearson was usually coy when describing the origins of Biometrika, giving the reader the impression that it was the result of a situation that was a disgrace to science. For example in the "History of the Biometric and Galton Laboratories" written in 1920, Pearson wrote only that obstruction to publication had led to his and Weldon's decision to start their own journal.[26] In the trust deed for Biometrika set up in 1935, Pearson provided a short history of the journal. Of its origins Pearson wrote only that "certain occurrences connected with the publication of papers by the Royal Society" led Pearson, Weldon and Galton to form the journal.[27]

Biometrika was initially published by Cambridge University Press, and a long series of letters between Pearson and the Press's representative (usually Richard Wright) reveal the struggles involved. At the time, there were several different types of agreements by which the Cambridge University Press would publish materials. Under some conditions the Press would accept all financial risks in return for a substantial percentage of the expected profits. At other times it would agree to publish on a straight commission basis.

For certain endeavors which did not promise great profits, but which were deemed worthy of publication for their value to society, the Press would agree to publish provided the proprietors insured against loss by guaranteeing a certain number of subscriptions and by depositing a guarantee sum on account.

In early 1901, Pearson and Weldon began soliciting promises of subscriptions. Meanwhile, Pearson negotiated with Cambridge while Weldon, who was by this time at Oxford, negotiated with the Clarendon Press. Initial prospects were decidedly gloomy. In February Weldon was pessimistic as he wrote to try to convince Pearson that they should publish at least one volume even if the venture was doomed.

> I have only <u>nine</u> promises! We shall have to consider whether we ought not to publish one volume anyhow....I hope you will agree that we should do so. I do not think the unpleasantness of failure ought to prevent it, and I do not think the disadvantage of burying papers in a dead journal (if the worst came to worst) ought to be very great.[28]

By May, negotiations were coming to fruition with <u>both</u> presses. Cambridge replied favorably to Pearson and Weldon's proposal for a "new quarterly journal of biological statistics." They were willing to publish it provided Pearson and Weldon could promise 100 subscribers and put up a guarantee sum of 100 pounds. Cambridge representatives had, however, caught wind of Weldon's continuing negotiations with Clarendon. They

politely but firmly scolded Pearson and Weldon. Shopping around for a better offer and running simultaneous negotiations was all well in good in a venture for profit, but to do so while asking the Press to publish at a probable loss for the good of science was simply bad form. Accordingly, Cambridge insisted that it must, in good conscience, stand aside until negotiations with Clarendon were concluded one way or the other.[29]

When, however, Clarendon, whose offer required a 200 pound advance guarantee, took exactly the same position, Pearson and Weldon smelled collusion. Each fired off salvoes to the respective publishers.[30] Pearson and Weldon played the injured party but they were stuck--they had to choose between the two presses before either one would make a serious offer. Weldon preferred the Clarendon Press because of its reputation for quality publishing and for its proximity to Oxford, but he left the decision to Pearson.[31] Pearson chose Cambridge, and on 18 June the syndics accepted a proposal and sent Pearson a draft of an agreement for publication of a quarterly journal to be sold at 30 s/net per volume, with four numbers in a volume and the individual numbers selling for 10 s/net. The agreement would be enacted if 100 subsribers were guaranteed and 100 pounds deposited.[32]

Pearson and Weldon raised the guarantee with help from Francis Galton, W.R. Macdonell and Lord Parker, and intensified their efforts to solicit both subscriptions and articles. Their efforts met with modest success as replies and subscriptions came in from both individuals and institutions not only in Great Britain, but from the continent and from America. Subscribers to the first volume included some notable individuals such as E.B. Tylor, the Oxford ethnologist, Henry Crompton and Richard Mayo Smith from Columbia University in New York, Walter F. Wilcox from Cornell University, and Rudolph Martin from Zurich. Letters of subscription came from institutions such as the Department of Animal Biology at the University of Minnesota, the Illinois State Laboratory of Natural History in Urbana, the General Library at the University of Michigan, the United States National Museum, the American Museum of Natural History in New York, the American Association for the Advancement of Science, the University of Chicago, the Marine Biological Laboratory at Woods Hole, the Missouri Botanical Garden, the Konigliche Bibliothek in Berlin, the University of Gottingen, and the Royal Swedish Academy of Sciences. In total, Pearson and Weldon met the 100 subscriber proviso with three to spare.[33]

Immediately after publication of the first number of Volume 1, Pearson and Weldon began making

preparations for a second number. Pearson also began haggling over the precise details, interpretation, and accounting procedures of the publication agreement. Pearson engaged in a continuous correspondence, at first almost daily, with the representative of Cambridge University Press, usually Richard Wright. Their letters stretch from 1901 through 1919 and provide an interesting and often amusing account of what it must have been like to deal with Pearson on a daily basis. Pearson was not satisfied with minimum subscriptions and, beginning with the second number to Volume 1, he included a sheet of abstracts to that and future numbers along with a form at the bottom asking readers to send in names of other potential subscribers. By February of 1902, the total number of subscribers had risen to 130. By May of that year the editors reached an advertisement exchange agreement with the anthropological journal Man and were receiving similar proposals from specialty journals such as the Quarterly Journal of Microscopic Science.

The exact figures regarding the financial state of Biometrika are difficult to determine. Pearson's constant haggling precipitated constant revisions of accounting procedures. Richard Wright, sounding both weary and desperately hopeful, wrote Pearson in June of 1902: "I think now everything is settled."[34] He was

wrong. Nothing was ever completely settled with Pearson. Nevertheless, the Press was reasonably happy with the state of affairs. While Volume 1 resulted in "a considerable loss even after payment of the guarantee" (a loss of approximately 60 to 70 pounds), they expected Volume 2 to do much better, and agreed to publish Biometrika for a further period of two years under the same basic agreement.[35] By February 1905, subscriptions stood at 201 and in March of that year the Press extended the agreement for an additional two-year period, covering Volumes 5 and 6. This in spite of the fact that the endeavor was still running at the same level of loss.[36]

In 1906 Karl Pearson became sole editor of Biometrika. The change was precipitated by Weldon's unexpected death from pneumonia in April. In terms of the actual editing of the journal, the change was essentially superficial. Pearson had from the beginning done almost all of the actual editing. Two years before his death, Weldon had written to Pearson saying that "Galton agrees with me that sooner or later (and rather sooner!) you will have to appear as what you are--the true and only editor."[37] The need to acknowledge Weldon's death also provided Pearson with a convenient opportunity to drop C.B. Davenport's name from the title page. Davenport had ceased to be an active participant

since his falling out with Pearson and Weldon over the issue of Mendelism.

By the end of 1906 the journal was catching on. Orders for back editions were such that Volumes 1 through 3 now showed a profit, and by October of 1907 the venture as a whole was "near the break even point."[38] The prosperity did not last long. The outbreak of World War I curtailed subscriptions both at home and abroad, as both printing and labor costs skyrocketed. As a result, the guarantee fund was quickly exhausted. The Press wrote Pearson that, in the face of such heavy losses, the former agreement would no longer do, although they were willing to continue to publish Biometrika "on commission" and at "the proprietors expense." Biometrika was published by Cambridge on this basis until 1922.[39] Pearson and Weldon's widow used their own money to sustain publication through the war years.[40]

Post-war recovery was slow. At his speech at the third annual Galton Dinner in December of 1922, a disheartened Pearson reported that the inadequacy of printing and publication funds was such that he could not be certain of his ability to continue publishing Biometrika.[41] By the following year, however, a much-cheered Pearson delivered a tongue-in-cheek report concerning his new vocation as a full-fledged publisher.

Pearson had taken over full publication duties and was issuing the journal from his office, only paying Cambridge for piece-work printing. The combination of cost reductions resulting from this change, and resurgent back-orders for earlier editions from recovering and restocking universities and libraries, put the journal in the black.

Pearson retained sole editorship of Biometrika even after his retirement, in 1932-33, from his position as head of the Department of Applied Statistics, issuing the journal from a small office lent him by D.M.S Watson in the Department of Zoology. Not until 1935 did Pearson begin to share the load (although not the control) of publication, as he set up a Trust Deed that named himself and four of his most trusted associates--W.P. Elderton, David Heron, J.F. Tocher, and his son E.S. Pearson--as trustees.[42]

Upon Karl Pearson's death in 1936 his son became managing editor. Egon Pearson retained this position until 1966, six years after his own retirement as head of the Department of Statistics at the age of 65. He remained editor of auxiliary publications until 1975. The Pearson family link to Biometrika spanned three quarters of a century.[43] Today, Biometrika is still published by the Biometrika Trust Fund at University College, London.

## Biometricians

Karl Pearson's make-shift research institute was home to an amazing assortment of people and projects whose output was characteristic of both middle-class British industriousness and Karl Pearson's unique vision. In addition to Weldon, biometricians who populated the twin laboratories in the 1890s included G.U. Yule, whose disagreement with Pearson over the meaning of the contingency table is outlined in Chapter 1 above, and Alice Lee, a lecturer at nearby Bedford College who became one of the most productive and constant members of the "biometric school" and the first person to take a Doctorate of Science under Pearson.[44]

The first Drapers' Company Grant, initiated in the Summer of 1903, brought increased financial support and, therefore, increased production. In his first report to the Drapers' Company in November of 1904, Pearson pointed to the assistance provided by the appointment of Dr. Lee "as a computer" and by the arrival of additional "paid calculators." Pearson was also able to attract a steady stream of young "Cambridge wranglers." The result of such assistance was the completion of several projects that had been percolating for quite some time. For example, an investigation of "the inheritance of moral and mental characters in man," was

completed and published as the Huxley Lecture of 1903.[45]

The rediscovery of Mendel's work at the turn of the century intensified the biological vein of the group's work, stimulating an outpouring of works on heredity. Weldon and Ernest Warren carried out a series of laborious microscopic measurements to study inheritance and the effect of natural selection on snail populations. Soon thereafter the lack of a suitable permanent position forced Warren, a major contributor in the early days of the Biometric Laboratory, to emigrate to South Africa where he became the Director of the Natal Government Museum in Pietermaritzburg. W.R. Macdonell, who later became Lecturer on Biometry at the University of Aberdeen, and Cicely Fawcett published articles reporting on a series of craniological studies, while Amy Barrington, one of a series of young women recruited from Girton College in Cambridge, studied inheritance in greyhounds. Meanwhile, Pearson and Lee conducted a collaborative study of the inheritance of physical characters in humans.

The Biometric Laboratory also functioned as a consulting firm of sorts (albeit unpaid), and did work and prepared reports for governmental agencies. Examples of this type of work include a study of "Scottish Pauper Lunatic Rates" for the Scottish Lunacy

Commission, a report on time-fuses for the Royal Artillery, and a report on cancer statistics for the Cancer Research Laboratory. A related function of the laboratory was to offer advanced training to statistical researchers. Between 1902 and 1904, for example, the laboratory hosted representatives of the Indian Medical Service, the Metropolitan Asylums Board Service, and London Hospital.[46]

By 1909, the laboratory had developed in such a way that Pearson could classify its work into various categories. The first category concerned "routine work for the degrees in science and arts at the university." Pearson had a large load of duties in this area, but he used the Drapers' Grant to relieve himself of most of them, passing the load to a chief assistant named E. Cunningham who was paid out of the grant. The second category of activity concerned "higher teaching in mathematics and physics." With the help of his assistant, Pearson was able to fashion a two-year course designed to direct the advanced student into original research projects.[47]

Another category was related to "routine work for the engineering degrees of the university." This involved Pearson in supervising and assisting engineering students, but Pearson managed to channel this work into a series of publications entitled the

Drapers' Company Memoirs, Technical Series. Here such problems as the examination of stresses in crane and railway coupling hooks, the stability of masonry dams, and the theory of the design of metal arches were tackled.

The final category was the one which directly comprised the work of the Biometric Laboratory which was taking on an international flavor. Between 1903 and 1909, the laboratory was home to four post-graduate students from America, one from Germany, one from Japan, two from the Indian Medical Service, and others from Oxford, Cambridge, and the Scottish Universities.[48]

The journal Biometrika was issued from the laboratory, as was the second of the Drapers' Company Memoirs--the Biometric Series. This series included the extension of statistical treatment to "important problems in inheritance and sociology" and to "a wide class of biological cases in which the change in one characteristic influences a second, but not proportionally."[49] Plans were also underway in 1909 for the publication of a massive monograph on "albinism in man." Material collected by Pearson, in collaboration with E. Nettleship and C.H. Usher, included "upwards of 100 photographs, lithographs and color plates depicting albinos and their pedigrees in

European, Malay, Maori, Papuan, Indian and other races."[50]

The prodigious amount of data produced at the Biometric Laboratory that related most directly to eugenics was published in the Drapers' Company research series entitled Studies in National Deterioration. The object of this series was to further the study of "separate factors as to health, fertility and inheritance in man, which make for material fitness and racial welfare."[51] Pearson's series of articles on tuberculosis, which first embroiled him in the controversy with Yule over the proper interpretation of contingency tables, were published in this series.[52]

The last category of activity constituted the work done at the Francis Galton Laboratory of National Eugenics. True to the aim of its benefactor, the denizens of the Galton laboratory set out to "study the agencies under social control that may improve or impair racial qualities of future generations, either physically or mentally."[53] Among the most active researchers at the Galton Laboratory was David Heron, a Scotsman who investigated the inheritance of insanity. He later became director of a large insurance office and Pearson's favorite illustration of the benefits of a statistical education to the everyday business world. Edwin Schuster studied the inheritance of "ability", and

Ethel Elderton who, like Alice Lee, became a mainstay of the biometric school, analyzed the reliability of data gathered from the study of cousins in estimating the inheritance of family tendencies.[54]

Pearson collaborated with Amy Barrington, another Girton College recruit, on the question of the "relative influence of heredity and of home and school [environment] on defective sight."[55] All of the publications resulting from these projects appeared under the auspices of the Galton Laboratory's own memoir series, and the collective effort of the inheritance researchers resulted in a massive compendium of "material bearing on the inheritance of special characters (physical, mental or pathological) in man"--The Treasury of Human Inheritance--that Pearson edited.[56] As if this were not enough, Pearson was, until 1911, responsible for teaching graphics for engineering students and for practical astronomy. In the latter role, he even designed and built two small student observatories which still stand (but do not function) today.

In all, during the years 1903 to 1909, the two laboratories and their workers and collaborators produced 150 publications in eight different categories: mathematical and physical papers, astronomical papers, technical memoirs, theory of statistics, vital and

medical statistics, eugenics laboratory memoirs, biometric memoirs (including contributions to the mathematical theory of evolution, papers dealing with inheritance, craniological investigations, general anthropometric researches and biological memoirs), and "miscellaneous works."[57]

Work in the laboratories from 1909 to 1914 was shaped by the establishment of the Department of Applied Statistics which was created through the combination of the Biometric and Galton Laboratories upon Galton's death in 1911. The department developed along research lines, "as an institution for investigating, by statistical methods, social, economic, eugenics, and medical problems."[58]

Between 1908 and 1914, 166 publications were issued by, or prepared in, Pearson's department. W.H. Salmon, a Cambridge man who was Head of the Mathematical Department at Northhampton Institute, wrote on meteorological phenomena and geometric analysis. Pearson`s assistant E. Cunningham, produced articles in electrodynamics and fluid studies. Pearson and another of his recruits from Girton College, Julia Bell, published papers in astronomy concerning the spectral analysis of variable stars. Collaboration between Pearson and David Heron produced nearly thirty papers on the theory of statistics. Pearson also produced

technological papers on ideal masonry arches and material stresses, and contributed heavily to the over forty papers on vital and medical statistics. The topics of these medical papers ranged from tooth decay to mental defects and tuberculosis.[59]

The biometricians who populated the twin laboratories from 1908 to 1914 also remained active in the application of statistics to biological problems, publishing nineteen memoirs including the works on albinism and on selection in plantlife. The main biological concern continued to be the study of inheritance. The biometricians investigated topics ranging from "skin color crossings" to "the separate inheritance of quantity and quality in cow's milk," the latter being Pearson's own contribution. Pearson also produced six memoirs on the mathematical theory of evolution, all concerned with some aspect of heredity, correlation, and the effects of selection.[60]

During this period, the biometricians expanded the scope of their craniological and anthropometric investigations. R. Crewdson Bennington and L. Isserlis produced most of the craniometry, while Julia Bell and Ethel Elderton contributed osteometric (long bone) studies. The biometricians also undertook many "miscellaneous" projects, which included a third edition of part one of Pearson's Grammar of Science, Volume 1 of

his Life, Letters, and Labours of Francis Galton, and a bibliography of current literature in biometry and eugenics prepared by Julia Bell.[61]

The war years wreaked havoc on both of the laboratories and both recovery and publications came slowly and painfully. Between the years of 1914 and 1918, the biometricians still managed to issue 62 "memoirs," but they represented work done at an earlier time as a dwindling publication budget and the demands of the war work prevented any new initiative.[62] From 1919 to 1921 efforts to bring the biometric program back up to its former level of production yielded scant success. Because insufficient funding made it difficult to recruit and retrain a staff, the publication output mostly consisted of the finishing up of "war work" and with the publishing of series of "manuals dealing with both the art and theory of calculation"--the Drapers' series entitled Tracts for Computors.[63]

The development of the physical aspect of the research institute--the new Bartlett Building and the training of new staff took most of the energies of the still financially strapped school during the years 1922 through 1924. They still managed 60 publications during this period, however, and slowly the research program of Pearson's unwavering vision began to pick up speed once again. From 1925 to 1929, the latest version of the

biometric school again published 130 original papers. Several of the names of the contributors would have been familiar to pre-war readers--Major Greenwood, Ethel Elderton and Julia Bell--but there were also a new set of stars in the next generation of biometricians.

Most notable and productive in the new group were Dr. Percy Stocks, who took over the medically oriented investigations, G.M. Morant, who became the most frequent contributor to the craniometric wing, and the newest star in the area of mathematical theory, who in the 1930s would collaborate with J. Neyman to make major contributions to statistical theory, Egon Sharpe Pearson--Karl's son. Through all the changes, however, the most prolific contributor remained "K.P." himself.[64]

Although the publication activity level eventually returned to pre-war levels and primary areas of interest remained constant, the war did fundamentally change the character of the biometric school. The war had exhausted much of the fervor for eugenics and despite Pearson's efforts to fight the trend with ventures such as The Annals of Eugenics which began publication in 1925, the content of the work and publications of the post-war biometricians quickly began to develop along the more purely theoretical lines that characterize them today.[65]

## The Search for Identity

Each author of the two histories examined in Chapter 1 breathed new life into the dead letters that constitute the historical record of biometry by relating them to a different set of texts, creating a different context with which to read them. This Chapter returned to those dead letters with the expressed intention of ignoring the narrative constructs of previous historiography and producing new strands of narrative which provide greater detail about the various activities of the biometricians. The narratives produced here have, of course, still only scratched the surface. Each strand, and many more like them, could be followed deeper and deeper, forcing the historical record to yield more and more detail. This pursuit alone, however, would not bring us any closer to an authentic biometry, or Biometrika, or biometricians. In fact, by themselves, these narratives are not historical. The search for ever greater detail alone is an antiquarian pursuit. Historical writing attempts more.

Historical writing reinvests the historical record with fresh significance by integrating it into a dialogue about current issues. The insight gleaned from the critical reading of the historiography of biometry in Chapter 1--that each major strand of historical work

that has dealt with the history of biometry has created a different historical identity for biometry in order to tell a larger story that has contemporary significance--must be seen, therefore, not as a criticism of those histories, but as an insight into the very nature of historical writing.

By itself, the subversion of the main concerns of existing historiography and the search for, and explication of, a greater level of detail has not, and cannot, lead to an authentic biometry. The authors that produced the historical record of biometry cannot be resurrected. Indeed, the make-up of the historical record is determined not by dead authors or actors, but by the concerns of the larger narrative in which historians place it. Decision as to which texts will speak for the dead biometricians and which will remain silent depend on what kind of story the historian has conceived. Biometry has a historical identity only within that story.

If there is reason to return to the dead letters of the biometricians, it does not lie in a search for authenticity. They should only be (re)read if the historian has a new tale to tell--one worth telling now. I believe that there is such a tale and I have been telling it for two chapters. It is a story about the role of story-telling in the creation of knowledge.

In this story, the biometricians are cast as prolific producers of knowledge. They produced ideologically charged knowledge about the nature and development of human beings. Along the way they created more than a research institute and a publication vehicle, they fashioned and maintained an identity, both for themselves and for the objects which they probed. These identities were fashioned within a dialogue between author and audience (real or imagined). The story that will constitute this history of biometry is a story of the role that such a dialogue plays in the creation of knowledge--both historical and scientific.

## Notes

1. Karl Pearson, MS of his speech at the presentation of his bust by H.R. Hope Pinker, File 32, Karl Pearson Papers, University College London Library, London. The MS is undated, but the bust was finished and exhibited in 1924. See also, E.S. Pearson, "Karl Pearson: Some Aspects of His Life and Work," Part II, Biometrika 29 (1936-38): 161-248, p. 222.

2. This was the one element of Pearson's prophecy that did not come true. When I visited the archives in January and February of 1992, I found the bust sitting unobtrusively in the corner of the reading room of what is now the Pearson building.

3. Karl Pearson, "History of the Biometric and Galton Laboratories" (printed by the College in conjunction with an appeal for funds), 1919-1920, Folder 247, Karl Pearson Papers, University College London Library, London, p. 1. Pearson was appointed Professor of Applied Mathematics and Mechanics in June of 1884 at the age of 27.

4. Ibid.

5. Ibid.

6. Ibid.

7. The building, then called the Bartlett Building, stands there today and currently houses the Department of Statistics. It was renamed the Pearson Building in 1980.

8. K. Pearson to F.J. Weldon, 25 December 1912, as quoted in E.S. Pearson, "Karl Pearson: Some Aspects," p. 188.

9. Ibid.

10. Ibid.

11. Karl Pearson, "History of the Biometric and Galton Laboratories," p. 3.

12. Ibid.

13. Ibid.

14. Ibid.

15. Ibid. p. 4.

16. Karl Pearson, MS of Speech Delivered at the Celebration of the Opening of the Bartlett Building, 4 June 1920, Folder 34, Karl Pearson Papers, University College London Library, London, p. 1.

17. Karl Pearson, Report to the Drapers' Company, 28 April 1921, Folder 233, Karl Pearson Papers, University College London Library, London, p. 1.

18. Karl Pearson, Report to the Drapers' Company, 29 February 1924, Folder 233, Karl Pearson Papers, University College London Library, London, p. 1.

19. Ibid.

20. Karl Pearson, MS of speech delivered at the 13th annual Galton dinner, 22 January 1932, Folder 32, Karl Pearson Papers, University College London Library, London, p. 1.

21. Provost to Pearson, 24 November 1932, Folder 38, Karl Pearson Papers, University College London Library, London.

22. Provost to Pearson, 30 November 1932, Folder 38, Karl Pearson Papers, University College London Library, London.

23. Pearson to Principal, 3 February 1933, Folder 38, Karl Pearson Papers, University College London Library, London..

24. Lord Macmillan to Pearson, 22 March 1933, Folder 38, Karl Pearson Papers, University College London Library, London.

25. Charles Jasper Sisson to Pearson, 15 December 1932, Folder 38, Karl Pearson Papers, University College London Library. London.

26. Karl Pearson, "History of the Biometric and Galton Laboratories, p. 3.

27. Trust Deed for Biometrika, May 1935, Folder 347, Envelope 5, Karl Pearson Papers, University College London Library, London, p. 1.

28. Weldon to Pearson, 8 February 1901, Folder 891, Karl Pearson Papers, University College London Library, London.

29. Cambridge University Press to Pearson, 21 May 1901, Folder 347, Envelope 1, Karl Pearson Papers, University College London Library, London.

30. Weldon to Pearson, 29 May 1901 (with enclosure of a letter from Clarendon Press to Weldon), Folder 891, Karl Pearson Papers, University College London Library, London.

31. Weldon to Pearson, 13 May 1901, Folder 891, Karl Pearson Papers, University College London Library, London.

32. Weldon to Pearson, 19 June 1901, Folder 891, Karl Pearson Papers, University College London Library, London.

33. Correspondence from, and lists of, subscribers to the first volume of Biometrika, Folder 397, Envelope 3, Karl Pearson Papers, University College London Library, London.

34. Wright to Pearson, 19 June 1902, Folder 347, Envelope 1, Karl Pearson Papers, University College London Library, London.

35. Wright to Pearson, 12 May 1903, Folder 347, Envelope 2, Karl Pearson Papers, University College London Library, London.

36. Wright to Pearson, 20 February 1905, Folder 347, Envelope 2, Karl Pearson Papers, University College London Library, London.

37. Weldon to Pearson, 9 January 1904, Folder 891, Karl Pearson Papers, University College London Library, London.

38. Wright to Pearson, 20 February 1905, 3 March 1903, 16 November 1906, and 21 October 1907, Folder 347, Envelope 2, Karl Pearson Papers, University College London Library, London.

39. Wright to Pearson, 15 October 1915, 28 October 1915, and 19 January 1916, Folder 347, Envelope 2, Karl Pearson Papers, University College London Library, London.

40. Trust Deed for Biometrika, p. 1.

41. Karl Pearson, MS of speech delivered on the occasion of the third annual Galton Dinner, 17 January 1922, Folder 32, Karl Pearson Papers, University College London Library, London, p. 6.

42. Trust Deed for Biometrika, p. 1.

43. For Egon Pearson's career, see M.S. Bartlett, "Egon Sharpe Pearson, 1895-1980," Biometrika 68 (1981): 1-12.

44. For Yule's controversy with Pearson, see also Donald MacKenzie, Statistics in Britain, 1865-1930, (Edinburgh: Edinburgh University Press, 1981), pp. 152-182; For Alice Lee, see R. Love, "Alice in Eugenics Land: Feminism and Eugenics in the Scientific Careers of Alice Lee and Ethel Elderton," Annals of Science 36(1979): 145-58.

45. Karl Pearson, Report to the Drapers' Company, 26 November 1904, Folder 233, Karl Pearson Papers, University College London Library, London.

46. Ibid.

47. Karl Pearson, Report to the Drapers' Company, 1909, Folder 233, Karl Pearson Papers, University College London Library, London.

48. Ibid.

49. Ibid.

50. Ibid.

51. Ibid.

52. For Yule's controversy with Pearson, see Mackenzie, Statistics in Britain, pp. 152-182; see also E.S. Pearson, "Karl Pearson: Some Aspects of His Life and Work, Pt. II, p.170.

53. This was Galton's definition of eugenics. It was often quoted by the biometricians, and is quoted here from Karl Pearson, Report to the Drapers' Company, 1909, Folder 233, Karl Pearson Paper, University College London Library, London.

54. Karl Pearson, Report to the Drapers' Company, 1909.

55. Ibid.

Ibid.

Ibid.

58. Karl Pearson, Report to Drapers' Company, 15 March 1914, Folder 233, Karl Pearson Papers, University College London Library, London.

59. Ibid.

60. Ibid.

61. Ibid.

62. Karl Pearson, Drapers' Company Report, 19 February 1918, Folder 233, Karl Pearson Papers, University College London Library, London.

63. Karl Pearson, Report to the Drapers' Company, 28 April 1921.

64. Karl Pearson, Report to Drapers' Company, 29 Feb. 1924 and 29 April 1929.

65. See E.S. Pearson, Karl Pearson: Some Aspects, Pt. II, p. 205, and MacKenzie, Statistics in Britain, p. 117.

# CHAPTER 3
# THE IMPORTANCE OF HISTORY

## Inheriting the Future: Pedigree, Legacy, and Context

Chapter 2 surveyed the vast array of scientific activities and publications produced by the biometricians and published in Biometrika. Scientific memoirs, however, were not the only genre of writing to appear regularly in the pages of Biometrika. Published alongside their scientific counterparts were numerous historical memoirs.

In general, the biometricians' historical writing was characterized by three main concerns. Their first concern was the historical lineage or pedigree of biometric science. In these pieces, the biometricians were portrayed as the rightful and even natural heirs to the stewardship of progress in science. The second concern was the historical development of science in general. In that narrative, the progress of science was in great danger, and only biometry, the natural progressive step in the evolutionary development of the "sciences of life," could hope to defeat the forces of dogmatism and reaction that threatened the future of science. Finally, the biometricians wove their

histories into a larger narrative of the progressive development of "man," evoking powerful and respected themes of late 19th-and early 20th-century British culture.

A good example of the nature of the pedigree that the biometricians fashioned can be found in the first image that appeared in the inaugural volume of their journal Biometrika. It was the image of an ancestor, raised to the level of an icon (See Figure 1). The icon confronted the reader of Volume 1 of Biometrika immediately, as it was presented as a frontispiece. The image consisted of a photograph of a statue depicting an elderly balding white man with a long beard and the slightly stooped posture of one who has born a heavy burden for a long time. The photograph situated the viewer at a noticeably low angle and its cropping mimicked the shape of a cathedral window. Its effect was to make the reader into a parishioner, gazing up at a patron saint. At the bottom of the photograph, the skeleton of some large dinosaurish beast could be glimpsed lying contentedly at the feet of the saint, tamed. The subject of the statue was not identified on the frontispiece. Instead, a motto appeared just below the photograph in bold print and in Latin. It read: IGNORAMUS, IN HOC SIGNO LABOREMUS. It was a motto of humility: "We are ignorant, in this sign we work." But


Figure 1. Frontispiece to Volume 1 of Biometrika, 1901-02.

it recalled both the motto of the Jesuits (In hoc signo--"In this sign," that is, in the sign of the cross) and the motto of the soldiers of the Emperor Constantine (In hoc signo vinces--"In this sign you will conquer").[1]

Textual reference to the icon was made only at the very end of an editorial that introduced Biometrika to its readers. The editorial concluded that "biometry is an instrument which can aid us effectively in our gropings after truth. Only let the spirit in which it is used be that of the master-mind, the ideal so well and faithfully portrayed in form and features on our frontispiece."[2] Thereafter, the motto was repeated in bold type.

The master-mind, the ideal, the subject of the icon, was Charles Darwin, whose features were easily recognizable to even the most casual peruser of Biometrika in 1901.[3] The icon, therefore, depicted the biometrician as a humble but tenacious laborer in a larger cause which began with Darwin. The biometrician of 1901 was expected to be both proselytizer and warrior whose mission was to conquer the realm of biological problems in the spirit and name of Darwin.

The frontispiece was followed by two introductory essays. One was an editorial in two parts which addressed the scope and spirit of Biometrika. Though unsigned, its authorial persona of "the editors"

outlined numerous aspects of the biometrician's identity. The biometrician was to be both a student of, and a contributor to, a new biology. The biometricians' work was to involve both the collection and publication of "biological data," and the spreading of the knowledge of statistical theory requisite for the scientific treatment of that data.[4]

In the explanation of the nature of biological data and the necessity of statistical treatment, distinctions between the new and old biologies were articulated. The science of morphology was presented as an example of biology in the past. Its icon, the reconstructed skeleton, was seen lounging tamely at the feet of Darwin. The guiding principle and central practice of morphology--the concentration on individuals and their relationship to a constructed ideal type--was identified as an old biological practice. In contrast, the starting point of the new biology was to be "difference." In the Darwinian theory of evolution by natural selection, the raw material upon which natural selection operated was "individual differences."[5] Accordingly, the data-collecting biometrician needed to be on the look out <u>not</u> for unity of type, but for aberration from type, for degrees of abnormality.

> The first step in an enquiry into the possible effect of a selection process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any given

> degree of abnormality with respect to that character, occur.[6]

The individual, the unit of analysis of the old biology, was to be replaced by "the race, or a statistically representative sample of the race," because the key to the "problem of evolution" was to determine the frequency of abnormalities, of incipient changes in a race.[7]

The new biological data (observed differences) and the new biological unit of analysis (the statistically significant sample of a race) were to yield results that were to be expressed in a language new to biology--the language of mathematics. The results of biometric investigation were to "take the form of a numerical statement."[8]

To speak of the mathematization of biology was to tap into well-acknowledged notions of physics as the queen of the sciences, whose methods and form were to be emulated.[9] But the editors of Biometrika were aware that there was resistance to mathematical methods among biologists who believed that numbers were too abstract to correspond to nature. To soothe these fears the editors offered this assurance:

> The danger will no doubt arise in this new branch of science [biometry] that--exactly as in some branches of physics--mathematics may tend to diverge too widely from Nature. The biologist, the mathematician and the statistician have hitherto had widely different fields of work. Each one of these fields is full of pitfalls, and

> when the worker amid living types wanders among symbolic forms, the mathematician by profession must give him a helping hand if he stumbles over a determinant or gets entangled in a differential. A like patience must be extended by the biologist to the mathematician when he makes blunders at which the morphological tyro would smile.[10]

Even in that reassurance, the reader was informed that the new biologist would be different from his field naturalist predecessor. The biometrician would "wander among symbolic forms."[11]

In addition to establishing a historical context which constructed biometry as a new age in biological science, the editorial also contained information about the nature of the biometrician as a new pedigree of scientist. The paragraph quoted above, for example, also stressed that the creation of a new biology would require an unprecedented degree of cooperation. To help foster such cooperation, a metaphor for operations, which the editors identified as an utterance of Francis Galton, was provided in the second half of the editorial, under the heading "The Spirit of Biometrika." According to the editorial, members of the new school of biometry were to think of, and conduct themselves, as members of a good English business firm. In the editorial's words,

> we want a scientific firm with a biologist and a mathematician as acting partners and a logician as consulting partner. Patient endeavor to understand each other's methods, and to bring them into harmony for united ends and common profit--this is the only method by which we can win for biometry a

> recognised place in the world of science and in the accepted academic curricula of the universities.[12]

In addition to fostering the necessary cooperation among scientific investigators, the formation of the biometric firm was to bring social benefits to the world of science, by creating fresh opportunity for "employment." Specifically, Biometrika would

> serve as a store house of unsolved problems for both unemployed biologist and mathematician...and strive to form a link between...[the] many men and women with the necessary training...who, without having the opportunity for initiating original work are not only competent but glad to assist with collecting box, camera or pencil...and those desiring their aid.[13]

The second essay after the frontispiece was a memoir signed by Francis Galton, simply entitled "Biometry." Here a second similar metaphor was provided. It was a metaphor that referred to that institution upon which the English business firm was modelled--the military regiment. Here the metaphor worked as both a model for the behavior of biometricians and for the creation of statistical data. Modern methods of statistics, Galton told the reader,

> commence by marshalling the [individual] values [members of a disorderly mob] in order of magnitude from the smallest up to the largest, thereby converting a mob into an orderly array, which like a regiment thenceforth becomes a tactical unit.[14]

Extending the metaphor of statistics as mob control, Galton offered the following explanation of these methods to the neophyte biometric reader:

> Conceive each [previously disorderly individual] value to be represented by an extremely slender rod of proportionate length, and the rods to be erected side by side touching one another, upon a horizontal base. The array of closely packed rods will then form a plane area, bounded by straight lines at its sides and along its base, but by a flowing curve above, which takes note of <u>every one</u> of the values on which it is founded, however immense their multitude might be.[15]

The image of the subjection of individual values to a discipline and order for utilitarian purposes was an image that dominated biometric discourse. It had implications for both the creation of biometric knowledge and for creation of the objects that represented it.

In this inaugural essay, Galton advised would-be biometrician that "workers in this new field...should close their ranks for the sake of mutual encouragement and support," and remember that "every investigator stands in need of expert criticism."[16] The expert criticism to which Galton referred would come from the equivalent of the regimental leader or head of the firm, that is, from the editorial persona of <u>Biometrika</u>.

With the spirit and scope of biometry illustrated by appropriate icons and metaphors, what remained was to ground the rationale for such a vision in a historical narrative. The first ingredient in such a narrative was

already in place in the notion of Darwin as "founder."[17] The editorial essay began to construct a historical context for biometry by combining, through analogy, the notion of biometry's ancestral relationship with Darwin and its emulatory relationship with physics. Specifically, the editorial asserted that biometricians were to Darwin as the French mathematicians were to Newton and as contemporary studies in electromagnetism were to Faraday.[18]

The analogy was not without problems. The French Newtonian mathematicians were so-named because they had used and extended Newton's invention--the calculus. Similarly, contemporary studies in electromagnetism could clearly be seen to be following paths of investigation originally opened up by Faraday. Darwin, on the other hand, openly and often professed his lack of aptitude and general dislike for mathematics. Aware of the importance of the analogy, the editors emulated a rhetorical strategy that Darwin himself had perfected in the Origin. They beat their imagined critics to the punch by raising the issue themselves. They even documented Darwin's trouble with mathematics by quoting a fragment of a letter Darwin had sent to Sir John Lubbock in 1857, concerning the statistical treatment of variation.

> You have done me the greatest possible service in helping me to clarify my brains. If I am as muzzy

> on all subjects as I am on proportion and chance,--what a book I shall produce![19]

Once raised, the objection was immediately defused by documenting the fact that Darwin held strong beliefs about the importance of statistical methods. Various fragmentary quotations were produced, until one was discovered which provided, in the view of the editors, "the strongest evidence of [Darwin's] consciousness that biometry offers the only possible solution of the problems of inheritance."[20]

After this build-up, the cited passage is something of a disappointment:

> I write now to say that I have been looking at some mongrel chickens, and I should say one week old would do very well. The chief point which I am, and have been for years curious about, is to ascertain whether the young of our domestic breeds differ as much from each other as do their parents, and I have no faith in anything short of actual measurement and the Rule of Three.[21]

Nevertheless, the editorial continued unabashed, and asserted that "these words prove fully Darwin's consciousness not only of the need of measurements, but also of arithmetical work upon such data in the case of heredity. They may well serve as a motto for Biometrika and for all biometricians."[22]

The iconization of Darwin as a committed, but problematic, founder of the new science of biometry had the effect of casting the biometric reader as a devotee of a new science which both followed from, and sought to

surpass, the great work of Charles Darwin. It also identified the editorial persona of Biometrika as a specially ordained acolyte, who alone possessed the knowledge to guide the practitioners (often referred to by Pearson and Weldon as "the Brethren") as they spread the "gospel" of the new science.

The rest of the historical context created for the reader of Biometrika in Volume 1 was located in Galton's essay. It came in the form of two stories--oral histories of sorts--"narrated" by Galton in the form of personal reminiscences. In the same manner that the metaphors referred to earlier were described as having been uttered by Galton, this story is passed on as a grandfather would pass on family history to his grandchildren.

Two representations of Galton, which served as frontispieces to Volume 2, later reinforced the grandfatherly image. The first was a photograph showing Galton in profile (see Figure 2). It depicted a serious man of great intellect. It was not a warm image. The man in the photograph did not look like he was easy to approach--certainly not with trifles. But his signature appeared below the photograph, along with a salutation which assured the reader that he was "sincerely yours, Francis Galton." The second representation was a sketch depicting Galton at work (see Figure 3). Here his head


Figure 2. Photograph of Francis Galton. First Frontispiece to Volume 2 of Biometrika, 1902-03.

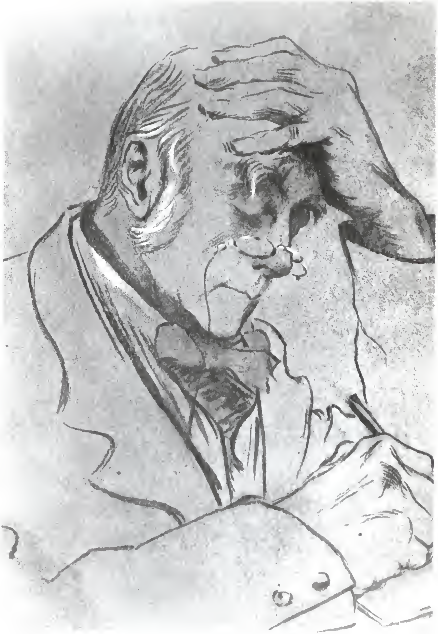


Figure 3. Sketch of Francis Galton. Second Frontispiece to Volume 2 of Biometrika, 1902-03.

rested on his left hand, while his right hand held a pencil, idle on a pad. His eyes were closed, perhaps in intense concentration. But the sketch lacked the intensity of the photograph. The man in the sketch looked older. The wrinkles around his eyes and cheekbones were more prominent. In the sketch, Galton was wearing spectacles which were balanced precariously upon the end of his nose.

The photograph presented an image of a powerful eminent man of science. The sketch depicted a kindly grandfather caught dozing--the kind of image that a child might catch of his grandfather after sneaking quietly into his study. Both images were important to the identity of the biometricians. Galton, as the grandfather of all biometricians, could be called upon to provide sage advice. At the same time, the biometrician, as progeny, naturally inherited both Galton's traits and reputation for making solid scientific contributions in a wide variety of fields in which he was often an interloper.

In the role of grandfather--keeper and dispenser of the oral history of biometry--Galton told two stories in his essay in Volume 1. Neither was about biometry as such. The first was about the reception of Darwin's theory of natural selection, and the second recounted a tale which was previously passed on orally to Galton by

one of the participants about the founding of the Geological Society of London.

Having firmly secured Darwin as the founder, in fact and in spirit, of the new biological science of biometry, it followed logically that the history of the reception of Darwin's theory of natural selection would be part of the historical context of biometry. Galton rendered that history in the following way:

> It is astonishing to witness how long a time may elapse before new ideas are correctly established in the popular mind, however simple they may be in themselves. The slowness with which Darwin's fundamental idea of natural selection became assimilated by scientists generally, is a striking example of the density of human wits. Now that it has grown to become a familiar phrase, it seems impossible that difficulty should ever have been felt in taking its meaning. But it was far otherwise, for misunderstandings and misrepresentations among writers of all classes abound during many years, and even at the present day occasional survivals of the early stage of non-comprehension make an unexpected appearance.[23]

In contrast, the presentation of the second story, a borrowed history, may have seemed odd, but its message was strikingly similar. It was, Galton told the reader, a story told to him long ago in the 1890s by Mr. George Bellas Greenough, F.R.S. It was a story which, as Galton informed the reader, he had been reluctant to tell, fearing that it might give pain to someone. But now that it had been "nearly a century" (the story began in 1806-07), Galton felt that there could be "no harm in

digging up and bringing to light a buried but amusing fact."[24]

To make Galton's long story shorter, it seems that young Mr. Greenough and a few of his friends approached Sir Joseph Banks, then the president of the Royal Society , with a list of questions which they thought went right to the heart of the debate between the competing geological theories of the day, and asked that those questions be "circulated under official sanction among suitable persons, including foreign correspondents of the Royal Society." Sir Joseph, whom Galton described as "almost an autocrat" who exercised his office "despotically", was apparently not content with an emphatic "no," and dismissed the young upstarts with the warning that "a few fools could ask more questions in half an hour than wise men might answer in years." The righteously indignant young fools started their own society which became the Geological Society of London.[25]

Together these two historical narratives contributed important elements to the historical context that was being constructed for biometry. In these stories, the biometrician was situated on a historical time line which originated with Darwin and had now arrived at the point of a new phase in the history of the reception of natural selection theory. The lessons

of the past taught the biometrician that there might always be reactionary opposition to new science which would both misunderstand and misrepresent, but that all such opposition would one day be seen as an example of an early stage of noncomprehension and as an example of the density of human wit. Furthermore, biometry, as ground-breaking science, was likely to face the opposition of the autocrats and despots of the power structure of the old biology, but like the early struggles of the Geological Society, this episode too would one day be the subject of a harmless and amusing anecdote.

What then was the moral of grandfather Galton's tales? It was that the future of biometry was secure, but only as long as the biometrician understood the historical context of the "present" moment--the moment of the reader. The position of "present day" biometry was precarious. There were stern tests of ruthless competition for the fledgling firm to face, and ferocious battles for the besieged regiment to wage. Victory would come, but only through unity of purpose and uniformity of action.

Each aspect of the identity of the biometrician fashioned in the pages of the inaugural volume of Biometrika reinforced the same message. As descendants of Darwin and Galton, the biometricians were to exhibit

the same sort of humility; taking guidance and admitting temporary ignorance in order to battle permanent and despotic ignorance. As practitioners of a brand new biology, each and every biometrician was a novice or, more accurately, a neophyte. Finally, located at a precarious historical moment, biometry required a voice. A voice which spoke with absolute unity and uniformity. Biometrika was that voice.

## Acquiring a Past

Four volumes after its debut, the title page of Biometrika changed significantly. The unexpected death of W.F.R. Weldon (on 13 April 1906) at the age of 46, and Pearson's disagreement with C.B. Davenport about the significance of the "rediscovery" of Gregor Mendel's work, necessitated the change. The title pages of Volumes 1 through 4 read: "Biometrika: A Journal for the Statistical Study of Biological Problems. Edited, in consultation with Francis Galton, by W.F.R. Weldon, Karl Pearson, and C.B. Davenport." Volume 5, covering the years 1906-1907, read: "Biometrika: A Journal for the Statistical Study of Biological Problems. Founded by W.F.R. Weldon, Francis Galton and Karl Pearson. Edited, in consultation with Francis Galton and in collaboration with C.B. Davenport, W.R. Macdonell, W. Palin Elderton, and Raymond Pearl, by Karl Pearson." The title page remained this way for Volumes 6 and 7,

but beginning with volume 8, all mention of "collaborators" was dropped.

In the context created by the new version of the title page, Weldon, Galton, and Pearson were elevated to the status, previously reserved for Darwin alone, of "founder." It was a double promotion for Weldon and Pearson who were originally depicted as descended from Darwin through Galton. The sons had now achieved the status of equal (and founding) partners in the firm. In this way, Biometrika's reader-contributors (themselves now identified as progeny of Weldon and Pearson) were informed that "grandfather" Galton's role in the firm was an increasingly symbolic one. In contrast to Weldon and Pearson's promotion, Davenport was demoted to the dubious status, especially in a discourse laden with military metaphors, of "collaborator."

Weldon's identity, which in the editorial essay of the inaugural volume was largely subsumed into and under Biometrika's editorial persona whose dominant component was Karl Pearson, appeared in Volume 5 as joint founder and co-editor who "will always be associated with the early history of biometry."[26] This shift in Weldon's identity with respect to the history of biometry brought about a correlated shift in the identity of the biometrician. In the historical context fashioned in Volume 1, the reader was situated at the moment of

conception of the science of biometry, whose history existed metaphorically, by way of an intellectual connection with the development of natural selection theory, and spiritually, by way of an analogy with the history of the Geological Society of London. In Volume 5, the reader looks back (guided by the editorial persona) at the "early history" of biometry, which predates the publication of Volume 1. The story of this early history was told in a biographical essay which served as a tribute W.F.R. Weldon.

The biographical essay appeared in Volume 5 of Biometrika, published in October 1906. It was preceded by the first frontispiece printed since the Galton icons appeared in Volume 2. It was an image of Weldon. In contrast to the earlier icons of Darwin and Galton, Weldon's image appeared after the title page and table of contents, suggesting that the collective identity represented by Biometrika itself now had first priority. The frontispiece was a photograph of Weldon (see Figure 4). It depicted Weldon at middle-age, as he might have looked shortly before his unexpected death. The photograph was cropped to show the man in the manner of a sculptor's bust. The torso appeared relaxed, but the head would have none of it. It was arrow-straight, erect but tilted slightly and purposefully forward, the way one does when walking into a stiff wet wind. The

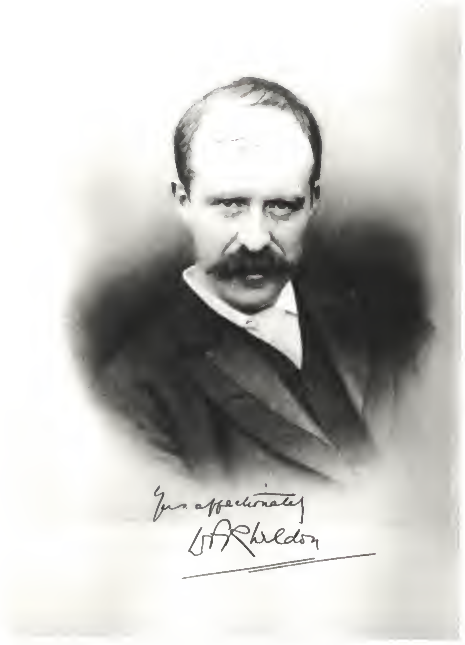



Figure 4. Photograph and Signature of W.F.R. Weldon. Frontispiece to Volume 5 of Biometrika, 1906-07.

prominent forehead encasing the formidable intellect was covered only by a wisp of hair. The eyes peered out from beneath a deep brow and stared directly into the reader's. The closing and signature--"Yours affectionately, W.F.R. Weldon"--revealed an impatient hand, but time was taken to underline the signature twice. The image created by the representation was one of an extremely dynamic and formidable man, with important things to do. He sat before the viewer, chaffing at the demands put on his time by the photographic process and by his disciples. He consented to meet these demands with "affection," but also with impatience.

The icon was followed by a fifty-two-page "memoir" entitled "Walter Frank Raphael Weldon, 1860-1906." The memoir was unsigned and was listed in the table of contents without an author's name. Like the editorial essay in Volume 1, the author of this memoir was the editorial persona of Biometrika, but footnotes and other textual clues clarified the identity of the often referred to "current writer" (and the "I" which occasionally bursts into the narrative) as "K.P." or "Karl Pearson."

Other contextual changes like those effected by the new title page were apparent in Pearson's tribute to Weldon. For example, Pearson directed Biometrika's

reader to a single paragraph which was said to contain the full expression of Weldon's ideas. The paragraph was the third paragraph of the editorial which opened Volume 1 of Biometrika, entitled "The Scope of Biometrika."

> The starting point of Darwin's theory of evolution is precisely the existence of those differences between individual members of a race or species which morphologists for the most part rightly neglect. The first condition necessary in order that any process of Natural Selection may begin among a race, or species, is the existence of differences among its members; and the first step in an enquiry into the possible effect of a selective process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any degree of abnormality with respect to that character, occur. The unit, with which such an enquiry must deal, is not an individual but a race, or a statistically representative sample of a race; and the result must take the form of a numerical statement, showing the relative frequency with which various kinds of individuals composing the race occur.[27]

In the new historical context created by Pearson's narrative in Volume 5, Weldon emerged as the author of the founding ideas of biometry. The new historical context merged with, and altered, the manifesto of biometry that was originally presented in Volume 1. Initially offered as a fresh statement of purpose without historical precedent, the biometric manifesto now became the product of a long, but unfinished personal evolution.

In presenting an account of Weldon's life to the biometric reader, Pearson extended the biometricians'

pedigree in both directions--presenting Weldon himself as the latest in the line, and extending the line of predecessors by constituting Weldon as the progeny of great men. In the process, Pearson instructed the biometrician not to be ashamed of "a certain amount of purely human hero-worship."[28] Then he described the line leading to Weldon, and instructed the reader to worship Weldon as Weldon worshipped his heroes (Francis Balfour, T.H. Huxley, and Francis Galton). The reader was to regard Weldon "not only as ideal thinker, but [as] an ideal character."[29] Pearson's account of the development of that ideal character, of Weldon's personal evolution, revealed much about his conception of the development of the ideal biometrician. This will be examined in Chapter 4. Here the focus remains on the creation of the general historical context of biometry's development fashioned by the editors' historical writings.

In his account of Weldon's life, Pearson told a more explicit story of the origins of Biometrika. The story began in the summer of 1899 with Weldon offering the "criticism, suggestion, and encouragement, in which he never failed," to Pearson who was working on poppies and developing a theory of "homotyposis, namely, the quantitative degree of resemblance to be found on the average between the like parts of organisms."[30] Weldon

sanctioned Pearson's work as "the big problem which all poor biologers have been trying for ever so long." The powers that be among these "biologers" did not, however, receive Pearson's work (submitted to the Royal Society in October of 1900) so favorably. In Pearson's words: "It was soon evident that the attitude of the [Royal] Society with regard to biometry was undergoing considerable change."[31]

Before giving the details, Pearson alerted the reader to the fact that the episode about to be recounted, that of the Royal Society meeting of 15 November 1900, definitely marked "the immediate cause of the proposal to found this Journal (Biometrika)." The narrative of the meeting mirrored that of the borrowed history--the story of the founding of the Geological Society of London--presented as an origin story in Volume 1. A group of young idealists (although in this case it is the science rather than the men who are particularly young) approached the power brokers of their discipline with a genuine offer of enlightenment and were treated unfairly. In this case, Pearson recounted that "a detailed criticism of the paper by one of the referees was actually printed by the Secretaries and issued to Fellows at a meeting, before the fate of the criticised paper itself had been notified, and before the paper itself was in the hands of those

present."[32] This unethical behavior, obviously designed to prejudice the minds of the Fellows, had the same effect on Weldon and Pearson as the rude and preemptory speech by old Sir Joseph had on young Mr. Greenough; namely it angered them enough to strike out on their own, determined that the world should know the truth. In Pearson's words, the actions of the Royal Society "confirmed the biometric school in their determination to start and run a journal of their own."[33]

In a quotation from Weldon, Pearson constituted for the reader the attitude of biometry's opponents, against which biometry's founders had to fight, as both unreasonable and archaic: "The contention that numbers mean nothing and do not exist in nature is a very serious thing, which will have to be fought. Most other people have got beyond it, but most biologists have not."[34] The passage served as yet another warning to present-day biometricians of the battles that must be waged by ground-breaking scientists.

Next, Pearson quickly recounted the events that brought the journal to fruition. Weldon proposed the idea for a journal in a letter of 16 November 1900. On 29 November Weldon sent Pearson a draft of a circular which elucidated the idea of the journal and which corresponded "fairly closely" to the editorial which

eventually appeared in Volume 1. When Weldon was unable to think of a title, the task fell to Pearson who sent the draft back to Weldon at Oxford with "the suggestion that the science in the future should be called Biometry and its official organ be Biometrika."[35]

The story of the birth and christening of the journal Biometrika established it as the voice of a young school that was greeted at its inception with a prejudice that came from closed thinking. In response the journal came to embody the truest values of real science--open and absolutely thorough inquiry--which it inherited from the temperament of its (now plural) founders. The two ideals were underscored in Pearson's conclusion to the origin story. In the process of telling the tale of the "christening" of the journal (the sacrament which establishes guardians to insure that the christened child will be inculcated with the proper values), Pearson provided an example of the "openness" that the journal was to embody:

> I did not see your letter yesterday until it was too late for you to have an answer last night. I like Biometrika and the subtitle. Certainly we ought to state that articles will be printed in German, French, or Italian. One may hope for stuff from anthropologers (sic), and [name omitted] for instance, certainly ought to be allowed to use his own tongue.[36]

In a footnote attached to a paragraph which recounted how and when the journal came to be published through the Cambridge University Press (by June 1901) and

extolling the care given to the publication by the university printers, Pearson pridefully drove home the point that absolute thoroughness permeated the project: "It deserves to be put on record that on more than one occasion 15 or 20 pages of figures have been set up without a single printer's error."[37]

Pearson concluded the origin story with a wish. It was a wish, shared by those "who believe that Biometrika came to stay and to fulfill a definite function in the world of science." It was a wish that "the name of the man who first formulated a definite proposal for a biometric organ may always continue to be associated with our title page."[38] In the articulation of the wish, Weldon became the icon which would forever inhabit the title page of Biometrika, infusing it with all of the ideological content of the historical context created by Pearson.

Pearson promulgated this identity of biometry as a youthful science facing unfair and unscrupulous opposition in other places as well. He wrote a "personal appeal to the American scientific world," which was published in the "Discussion and Correspondence" section of the 10 April 1903 edition of Science. In this essay Pearson wrote that he was turning to America for support because "in America, the novel, be it in science, politics or industry is not a

priori condemned as the undesirable or fatuous, which is its customary fate in Europe."[39]

In writing to his American audience, Pearson first attempted to arouse the reader's indignation by casting his students as abused and neglected children. Next, Pearson contrasted the open mindedness and modest assertions of the biometrician to the closed minded dogmatism of the opposition described earlier.

> None of our responsible biometricians claim that there is one way only of solving all biological problems, but solely that there is only one way, the statistico-mathematical method, in which certain, problems can be answered.[40]

When sympathy was sufficiently aroused, the seduction began. The mathematician who will take the biometric vow was promised nothing less than the future itself.

> In the case of the mathematician we have even more to offer than to the biologist,...in the future he may revel in vital phenomena....Perfect correlation, the causal nexus of the physicist, is only a special case of the general theory of correlation, which covers organic as well as inorganic relationships.[41]

Finally biometry promised to transform a vast array of fields so that all might work side by side.

> Anthropology, craniology, psychology and child study and pedagogy, as well as biology become fields of work crowded with new problems for the mathematician to tackle; nor must the worker in those fields look upon [the mathematician] as an undesirable alien. He comes to enrich their own domestic industries with new processes and show them how to handle new and powerful instruments of research.[42]

The biometric workers could be confident in their abilities to bring about such a transformation, because they were the product of a historical and evolutionary development that has selected them for the task.

## Contextualizing Particular Debates

The authors of biometric discourse also used historical writing to frame the context of particular debates. For example, Pearson's account of Weldon's involvement with Mendelism opened with a quotation from one of Weldon's letters to Pearson:

> About pleasanter things, I have heard of and read a paper by one, Mendel, on the results of crossing peas, which I think you would like to read. It is in the Abhandlungen des naturforschenden Vereines in Brunn for 1865. I have the R.S. Copy here, but I will send it to you if you want it.
> [October 16,1900.][43]

It may seem odd that Pearson chose to open an account of Weldon's struggle with Mendelism on such a light note, but Pearson's purpose was different from later historians of the controversy. In contrast to the histories of biometry in which the biometry-Mendelism controversy was the constitutive component of the biometricians' identity, Pearson's tale was understated. For example, there was no mention as to how Weldon was alerted to the paper. More startling to the reader who is prepared to hear a story of the single most significant event in the history of biometry is the fact that, after characterizing this period of Weldon's life

as dominated by information about Mendel's hypothesis, Pearson immediately began to downplay its role. Following the quotation from Weldon's October 16th letter, Pearson asserted that "for months the letters--always treatises--are devoted to snails, Mendelism and the basal things of life."[44]

Next followed two entire pages of barely interrupted quotations from Weldon's letters, none of which mentioned Mendel or his paper. Instead the reader was offered glimpses of Weldon's ambivalence towards Northern Germany and his love of snails:

> Have you ever been up here? It is not at all a bad little country when you are tired. We started simply to see the architecture at Lubeck, because neither of us knew the North German brick and wood church work. That was very interesting, then we came here for fresh air and quiet--and we found SNAILS.[45]

And of Weldon's view of art:

> You ought to come to Lubeck some day. You know so much about German art that I suppose the pathetic ugliness of it does not hurt you anymore?....You can't get beautiful art in a climate where people must wear clothes. Just as the northern idea of a portrait is a round face stuck on top of a heap of fine clothes, so the northern idea of a building is a thing with all its good simple lines disguised by silly excrescences.[46]

Pearson used one small anecdote among the many that appeared on these pages to elucidate further the distinction between the museum collection and the biometric collection which was to epitomize scientific collecting in the future. He began by presenting an

offhand anecdote from one of Weldon's letters in which Weldon complained that his efforts to collect statistically significant samples of various races of snails were being hampered by, among other things, the need to collect "isolated examples of pretty museum things, which are a joy to see, [but] teach one very little."[47] To the anecdote, Pearson attached the following footnote:

> One of the blows to Weldon, which resulted from his biometric view of life, was that his biological friends could not appreciate his new enthusiasms. They could not understand how the museum "specimen" was in the future to be replaced by the "sample" of 500 to 1000 individuals, not to be looked at through a glass, but to be handled, used, and if necessary, <u>used up</u>. They warned his pupils solemnly to give up this sort of fooling and take to the real business of the "biologer," if they wished for success.[48]

Pearson pressed the point home by adding, in the footnote, one further quotation from Weldon (dated 11 October 1900) which, in the language of religious warfare, clearly illustrated the kind of contest the biometrician was to expect.

> I told [name omitted] about these snails...and he wrote me an earnest letter, urging me to return to the pleasant way of describing beasts for the delight of the faithful. That is the real thing if you want to be popular. Go to the sea, and have a good time, and bring back a jelly fish that is bright blue.[49]

The point made, Pearson advised the biometric reader: "There is much missionary work yet to be done by the

biometricians, and Weldon's loss will make it still harder![50]

The trip through the Weldon's letters concluded, Pearson still did not move on to the promised glimpse of the flurry of intellectual activity over Mendelism. Instead, he told the reader of a "new factor" which had come into Weldon's life: the undertaking of the preparation of a critical bibliography of papers dealing with statistical biology to be published in Biometrika. The work on the bibliography, which Pearson described as yet another example of Weldon's temperament leading him to a patient and absolutely thorough method, provided the context in which Weldon's publications concerning Mendel were to be understood. Specifically, Weldon's thoroughness as exhibited by his work on the critical bibliography, was precisely the context in which Weldon's early enthusiasm for Mendelism was lost.[51]

In sum, the narrative structure of Pearson's history created an entire context in which Weldon's work on Mendelism was to be understood. In Pearson's narrative, Weldon's work on Mendelism was characterized by doubt born of a thorough reading of all of the important literature: "[Weldon] found that Mendel's views were not consonant with the results formulated in a number of papers he had been led to abstract." Next, those seeds of doubt blossomed into a telling insight,

as Weldon saw that "the definite categories used by some Mendelian writers did not correspond to really well-defined classes in the characters themselves." Soon, the insight was transformed into a vigorous critique as Weldon discerned "a certain looseness of logic, a want of clear definition and scale, an absence of any insight into how far the numbers reached really prove what they are stated to prove." Finally, the full weight of historical judgment came down on Weldon's side: "His attitude has been largely justified. The simplicity of Mendel's Mendelism has been gradually replaced by a complexity as great as that of any description hitherto suggested of hereditary relationships."[52]

By the time Pearson finally addressed Weldon's publications concerning Mendelism, they were already defined for the reader as early, and perhaps first, examples of the kind of critique which eventually forced a rethinking of the Mendelian hypotheses. The specific details of those hypotheses were never mentioned. In the historical context fashioned by Pearson's narrative, Mendelism was already part of a less enlightened past.

In the larger context of Pearson's narrative, the intensity and heat of the Mendelism controversy was characterized as just another example of the fate of the pioneer. The activities of the biometric laboratory, wrote Pearson,

> have been largely carried out in a boarder-land between the exact and biological sciences, where we are sure to meet, as pioneers, with hot criticism and even active opposition. It is the usual fate of the introduction of a new Columbus into old preserves, such as anthropology, medical and vital statistics, and astronomy.[53]

## Singing the Gospel in the Key of Progress

A constant theme of biometric discourse was that all fields of inquiry that might be called biological--the whole of the sciences of life--were to be reformed, and that the biometricians were the natural heirs to a tradition of scientific progress that guaranteed that reform. Obituary notices of Weldon's death show that this was well understood by contemporaries. One unsigned notice in the Athenaeum noted that "Dr. Francis Galton was, we think, the pioneer in applying precise measurements to biological phenomena....But Dr. Weldon, in association with Professor Karl Pearson, was for subduing the whole field of biology."[54] Another, written by Weldon's colleague--the Oxford astronomer H.H. Turner--underscored the same point, while grouping Weldon with great scientists of the past: "[Weldon] was aiming at nothing less than a revolution in biological study--at rending it quantitative rather than qualitative,...he was no more specially working on mice than Tycho Brahe and Kepler were specially working on Mars."[55]

The general theme of reform drew upon a central tension in late-nineteenth and early-twentieth-century British culture: a firm belief in the potential for progress in "Man" and civilization, particularly British civilization, and a growing fear that that potential was being squandered. The conjunction of these themes can be seen, for example, in a series of public lectures that Karl Pearson gave in Newcastle and Cardiff on Sunday's during the year 1903.

One lecture, entitled "Natural Selection," began by establishing the preeminent place of Charles Darwin in natural science, and by casting his theory into a narrative of organic progress.

> We all know the great drama of life unrolled before us by the genius of Charles Darwin. The lowly type microscopic in its dimension, slowly differentiated by the struggle with its physical environment, and constant friction with its fellow; then the diversity of types, each as it were an experiment fitting itself into special environment and to special function, succeeding or failing, stagnating or progressing.[56]

Pearson admitted to his audience that the drama that Darwin described did not seem to reflect their everyday experience, and he let them know that they were not alone by quoting from Lord Salisbury's address to the British Association for the Advancement of Science in 1894, in which his Lordship had voiced his own skepticism about the Darwinian view of life. Further, Pearson told his audience that they were absolutely

correct to make their everyday experience the criteria for their opinion about Darwin's notion of natural selection. Rather than rely upon analogies from selection in some exotic plant or animal, one should "look for evidence of natural selection working in man, in this race, in this country."[57] What Pearson asked of them was to allow him to lead them through a re-examination of their experience where the evolution of man was concerned.

What Pearson proposed was a step by step consideration of the evidence concerning each part of Darwin's theory, evaluating it with the most rigorous scientific methods--biometric methods. Pearson began by defining the first of three Darwinian hypotheses which would be tested. The hypotheses that "characters and organs are inherited from parent to offspring."[58] Before evaluating this hypotheses, Pearson situated the audience historically in a narrative of the history of science which cast biometry as the embodiment of progress and established a pedigree of innovation that ran from Darwin to Galton to the biometricians. "It is only in the last ten years that we have learnt through the work of Mr. Francis Galton to measure with scientific accuracy the quantitative strength of heredity."[59] Then Pearson took the audience through a step by step explanation of the methods now employed to

carry out a scientifically accurate quantitative evaluation of Darwin's claim. Slowly and patiently, with the demeanor and tone of the expert addressing an educated but lay public, Pearson explained how one plots a frequency distribution, constructs a histogram from the distribution data, converts the histogram into a frequency polygon or curve, and, finally, how one applies "certain mathematical tests" which check for "stability" in a population as a whole.[60] Giving enough detail to establish the authority that accompanied such expertise, but presenting no actual data with respect to Darwin`s first hypothesis, Pearson informed his audience that, on the basis of numerous studies, it had been concluded that "all characters are inherited in man with a degree of intensity lying somewhere between .45 and .5." There was no longer room for speculation, "the first stage in Darwin's theory is not hypothetical."[61]

The second hypothesis required by Darwin's drama according to Pearson was that "if individuals die earlier than others, [then] they have fewer offspring." Accordingly, biometric methods were used to calculate the correlation between age of death and fertility (or number of offspring). Pearson presented some examples of biometric studies of that correlation, and assured

the audience that Darwin`s second hypothesis was also confirmed.[62]

The final hypothesis required to show natural selection operating on humans was that the death rate had to be selective. In Pearson's words, it needed to be asked: "Do men with certain characters live longer than others? If we can show that they do, then natural selection is undoubtedly at work among men. The population will be progressively changing, and evolution has been once and for all established."[63]

In explaining how the problem could be approached, Pearson gave the audience a lesson in biometric logic. First Pearson explained that "the direct method" for determining what proportion of the death rate was due to selective pressure would be to "enquire in the case of any given organ or character, which did not sensibly change during life, whether its distribution among those dying under 40 years of age differed sensibly from those dying over 40."[64] This approach was prohibited by a multitude of problems such as the lack of adequate records and the difficult decision of which organ to pick. Fortunately, Pearson, now making the story personal, had a better idea.

> It occurred to me that we might approach the problem in a somewhat different manner. Could we not pick at classes of men who resembled each other--without definitely specifying the organs in which they resembled each other--and then investigate how far these classes did at different

> ages. Did the men of the same classes all tend to have the same duration of life?[65]

So the biometricians set out to study "relatives or men of like constitution," and found that such men "were liable to die--if the environment was much the same--at like ages." In fact, as the audience was told and shown with the aid of graphs, death "selects in 55 to 67 cases out of 100 when judged by paternal inheritance, and in 75 to 84 out of 100 when judged by fraternal inheritance." Therefore, Pearson concluded, "natural selection is not a hypothesis, it is a solid fact."[66]

The investigation, however, was not complete. One still needed to know whether the selection that biometric methods detected merely kept the population of Englishmen stable by periodically clearing away deviants, or continuously changed that population in a given direction. To answer that question, there needed to be some way to apply biometric methods over time. "We want," Pearson announced, "measurements of the men of the commonwealth-time to compare with those men of today."[67] Accordingly, biometricians were busy with projects such as the one being carried out by Cicely Fawcett and W.R. Macdonell, who were making cranial measurements on 1000 Cambridge undergraduates (a distinct class) and on some 3000 criminals (another class). These measurements were to be compared not only with each other, but with a series of measurements made

upon an archeological find of some English skulls historically dated from the seventeenth century.

The comparison between the two contemporary classes showed that "the ratio of breadth to length of the head of middle-class young Englishmen today is .78" For the criminal class it was .77". So, Pearson concluded, there did not seem to be a sensible difference. For the seventeenth-century skulls, however, the ratio of breadth to length was .75".[68] The audience was to understand that the difference between .77" and .75" was quite significant. "Now I do not think," Pearson asserted, "you could find any class of the population today with such a cephalic index! The middle classes say we are not like that! And the lowest elements say we are not like that![69]

Pearson not only wanted his audience to draw a particular conclusion from the numbers concerning the effects of natural selection on the race of Englishmen, he wanted them to see that it was obvious: "Now if a change in two points can be demonstrated to have taken place in the roundness of the English head in 250 years, I think we might see nothing unreasonable in differentiating the human and the gorilla skull from a common form in 250,000 years."[70]

Another lecture entitled "The Origin of the White Man" began, as usual, by setting the question, and

knowledge about the question, in a historical context. Pearson began by reminding the audience of a legend he assumed that they had all learned in their childhoods of the three sons of Noah from whom three distinct races developed--a white European, a copper Asian, and a black African. "It was a real problem for the Medieval theologian," Pearson declared, to know whether "Noah and eventually Adam was white, copper, or black....And it involved something further, for Adam was made in the image of God."[71]

From the seventeenth century onwards, Pearson said, appearances of "negroes with white skin, yellow hair and blue eyes" were interpreted as reversions to a white ancestor," and he cited the works of Maupertuis and Demanet.[72] From the Darwinian perspective, the question posed the problem of whether the races shared a single common ancestor, or evolved from separate origins. The Darwinian position was that consistent reasoning determined that the hypothesis should favor the single ancestor. "Surely the instinct that leads us to seek unity in the origin of life impels us to seek unity in the races of man."[73] But how could such a hypothesis be tested? Just as the focus on the inheritance of certain organs or characters was the key to testing the existence of natural selection in the population of man, the test of the hypothesis of the

origin of the white man had to rest on the inheritance of the organ or characteristic that defined the different races. The choice was obvious to Pearson:

> The fundamental differentiation which we have to account for almost certainly lies in the pigmentation. There are skeletal differences between negro and white, and there are certain more or less marked cranial differences; given a mixture of skulls of negro and white races, if you know it for a mixture, you could on the basis of general appearances sort them into their white and negro components, and if you have had experience of skulls you would probably be correct in all but a few cases,...[but] if you suspect a single skull of being negroid, you have no definite list [of characters] at present.[74]

Once it was decided that pigmentation or skin color was the characteristic that held the key to the production of races through natural selection, then the question became: "Where lies the greatest range of colour upon which nature might act by selection?" The answer, for Pearson, was dark skin, because white skin was white, but dark skin came in shades.

The conclusion was inescapable if one understood how the evolution of color coded races worked. In good Darwinian fashion, Pearson outlined the argument with analogies from the animal world. Pearson acquainted his audience with studies of birds, mice, and other more exotic forms of life and reported that in each case the source of color variation was the crossing of wild forms with an albino "sport."[75] In short, the mating of the hybrid offspring of albino sports and a normal strain

tended to produce "a general break-up of the normal pigmentation--a general instability of colour, which after inbreeding and selection can again be fixed in new fancy colors."[76]

Of course, albinos occurred in both "dark" and "white" skinned populations, but Pearson asserted that unlike the appearance of albino offspring in the "darker races of man," the progeny of white races "show no tendency to become deeper than the average of the race." Indeed, Pearson doubted the veracity of all reports of dark-skinned babies being born to white mothers, labelling all such reports "pre-modern."[77]

The conclusion of Pearson's "modern" inquiry contradicted the old biblical stories and the seventeenth-century theories. The conclusion of the modern, Pearson concluded, was that "we must...seek the `sub-human' in the locality where we find the darkest races."[78] By sub-human, Pearson meant the common ancestor that man shared with the ape, but the connotations created by the linkage of "sub-human" and "darkest races" were not accidental. Pearson concluded the lecture by addressing

> another point also on which much turns--we are only learning today how the extent of pigmentation affects the sensitory organs....It is conceivable that the intellectual dominance of the white races has been correlated with changes of internal, especially brain pigment, associated with his changes in external pigmentation.[79]

Making the origin of white man into a story of evolution from the darker races may have shocked religious sensibilities, but it was the perfect story for a culture fixated on the rhetoric of secular progress.

## A Post-War Tune

The experience of total war changed the context of biometric discourse. The war not only negated the institutional gains of the biometric program, it further shook British confidence in the inevitable progress of both "man" and civilization.[80] In the context of post-war uncertainty, a sense of biometry's historical identity remained an important ingredient in biometric discourse, but its maintenance required a new rhetorical strategy.

In 1920, Karl Pearson instituted the first annual Francis Galton Memorial Dinner. Such dinners were held each year for 14 years, as close to the January 17th death of Galton as possible. The Galton dinner was a night when the workers in the laboratory were reminded of, and instructed in, the history of the vision that constituted the biometric endeavor. At the first dinner in 1920, Pearson rose to toast Galton and to remind the disciples that "Galton has given us a tradition, if we can but keep it and understand it." The Galton toast was followed by a toast to the memory of "dead biometricians". In the memory of those fallen heroes,

contemporary biometricians were to find the meaning of the history of their science. They were to understand that biometry was a science which was in the process of transforming the entire array of life sciences, including anthropology, craniology, medicine, criminology, psychology, and physiology.[81]

In post-war renderings of biometry's historical identity, war metaphors were still used often, but the tone changed significantly. Where pre-war writings relied on war metaphors to create an enthusiastic and sacrificial spirit of conquest and glory for its recruits, the post war writings invoked the somber tones of the experience of war to create a sense of responsibility to those who gave their lives to secure the future.

For example, the "biometric dead" were always characterized as fallen warriors in the battle for progress in science. They fought against opposition which embodied any number of vices. Each warrior fell, usually before achieving his rightful acclaim, but knowing the cause was right and that he would be carried on by future generations of biometricians who would not forget him.

"Weldon," Pearson proclaimed at that first dinner in 1920, "fought a whole world of biologists,...and exhausted himself in the fight."[82] He also recalled

similar battles waged in the context of craniology and craniometry in which biometricians had been "abused" for papers published at the turn of the century which anatomists in 1920 considered classical memoirs. W.R. Macdonell, one of the earliest biometric workers in this area, was toasted that evening for having raised craniometry in England to the level of science, and for shaping its future.[83] The glass was also raised to Charles Buckman Goring, credited with reforming and shaping the future of the science of criminology, a task which in Pearson's words, involved a fight against "official senility as well as scientific pre-judgment."[84] The evenings round of toasts concluded with Pearson returning his audience minds to Galton and reminding them that their own contributions were still to be made. But also instilling them with confidence and pride by asking them to imagine "what a profound step forward science would have made had Galton been born a generation later and had such men for his lieutenants!"[85]

Pearson responded to the threat posed by post-war funding problems to biometry's institutional base by introducing the theme of hard-won professionalization into the history of biometry. The same year that the inaugural Galton Dinner was held, Pearson wrote a "History of the Biometric and Galton Laboratories" which

was printed and distributed by University College as part of a fund raising drive. In it Pearson offered another version of the origin of the biometric laboratory, locating it in the teaching of his own first course on the mathematical theory of statistics, which he characterizes as "probably the first course on the modern mathematical theory at all." The narrative is yet another story of humble beginnings, as those first lectures were given to a "laughingly small audience of two or three post-graduate students." The seed having been planted, "a school sprung up which has since been recognized as the Biometric School."[86] In describing the goals of the new school, Pearson juxtaposed them against an older school of political and social statisticians. The goals of Pearson's new school were "in general to convert statistics in [Great Britain] from being the plaything of dilettanti and controversialists into a serious branch of science, which no man could use effectively without adequate training."[87]

After a brief chronological summary of some of the events that led to the present laboratories and the Department of Applied Statistics, Pearson concluded that in all their endeavors the staff of two humble laboratories had remained true to Galton's belief that "until the phenomena of any branch of knowledge have

been submitted to measurement and number it cannot assume the status and dignity of a science."[88]

Just as a generation of British soldiers had given their lives, not for the love of war, but out of duty to a just cause, so the biometricians had taken up the gauntlet of scientific controversy. They had done it not for the pleasure of controversy, but because the biometricians felt duty bound to battle against the "inadequate and even erroneous processes in medicine, in anthropology, in psychology, in criminology, in biology, and in sociology," and to provide those sciences with "a new and stronger technique." Fortunately, the battle was nearly won "now that the old hostility"--the hostility of the old to the new--"is over and the new methods are being everywhere accepted."[89]

This story of conquering warriors having withstood the harsh rigors of battle to fulfill a responsibility to future generations demonstrates the responsiveness of biometric discourse to changes in the general text (or larger context) in which it was fashioned. Certain rhetorical strategies remained viable throughout. The characterization of biometricians as the embodiment of modernizing reform facing the reaction of the old guard was, for example, a constant element of the biometricians' identity. Other earlier moves, however, such as the characterization of biometricians as naive

innocents in Pearson's open letter to America, or as passionate raw recruits enthusiastically marching to glory in the editorial in Volume 1 of Biometrika, were no longer viable.

Continuing his "History" of 1920, Pearson presented several examples of successful biometric reform, each characterized as progress.

> Very few psychological journals now appear without biometric methods being used in one or more of the memoirs they contain....The anthropological journals both here and abroad now publish biometric investigations without hesitation, they are [also] frequent in the journals of medicine and hygiene....The first contribution of biometry to craniometry can now be spoken of by the anatomist as a `classical memoir' which the craniologist can not do better than follow.[90]

In this section of the narrative, economists, astronomers and sociologists were all yielding to the incursion of biometric methods--the latter saving itself from ruin by doing so, as the methods of the sociologist were not merely old, but "ancient."[91] Finally the "most striking illustration of the progress of biometric methods," according to Pearson's history, was "their frequent appearances in memoirs on genetics, heredity, and agricultural research, where the biologist of twenty years back had no word too strong in their condemnation."[92]

Pearson's narrative also gave a role to biometry in the history of reform and rationalization of the English university. Specifically, Pearson offered a short

intellectual history of the idea of a professor of statistics in an English university. His narrative situated the origin of the idea in the thoughts and desires of none other than Florence Nightingale. The legendary nurse had, Pearson contended, corresponded with Francis Galton about the advantages to the nation of having a professorship of statistics. In Pearson's story, only lack of funds prevented her from rationalizing higher education the way she had done for nursing.[93] With his research institute facing post-war financial difficulties, Pearson identified it with the most heroic reformer of the war era. The implication was clear: to allow Florence Nightingale's dream to be betrayed by lack of funding would be to a crime against all those that sacrificed everything in the war. As Pearson concluded: "No finer memorial could be raised to her memory than the endowment of a laboratory to fulfill this function."[94]

Having identified the reformist aspirations of the biometric laboratory with those of Florence Nightingale, Pearson brought the history up to 1920 by detailing the valuable work done by the laboratory staff during the war and by describing the potential of the new Bartlett Building. In the post-war context, however, the optimistic vision of the future was replaced with a sense of foreboding and a constant warning: "The war

has crippled institutions as well as men," Pearson wrote," and unless a fund is forthcoming for the permanent endowment of the joint labs, they will be left in a worse financial position than in 1914.[95]

As bleak conditions continued for the laboratories Pearson used the Galton dinners as a forum to proclaim an even broader place for biometry in the history of scientific thought. As Charles Darwin's ideas had "led to a reconstruction of all the biological sciences," Pearson proclaimed at the third annual dinner in 1922, so Galton`s ideas were "leading to a revolution in scientific logic [in] every branch of modern knowledge."[96]

By 1923, however, the biometric program's financial conditions and prospects for future development had improved remarkably. As a result, Pearson's annual reminiscences became jovial and the historical context he wove became softer and even conciliatory towards his opponents. For example, on the toastcard to the seventh annual Galton Dinner in 1926, which featured a photograph of the Hope Pinker bust of Weldon, a toast to the prosperity of the laboratories was added to the by now traditional toasts to Galton, to benefactors, to the Biometric Dead, to guests, and to the postgraduate workers.

On that occasion, Pearson even felt secure enough to take time to remember critics in an almost magnanimous way. The leader of the early British Mendelians, William Bateson, had died that year. Bateson had been portrayed in previous biometric histories as small-minded and unscrupulous--a creator of needless controversy that was in part responsible for Weldon's early death. But on this occasion Pearson spoke regretfully of the circumstances that had forced him to choose between Bateson and Weldon. Further, Pearson hinted at an eventual reconciliation between geneticists and biometricians.[97]

Pearson also spoke fondly of another critic that evening--the recently deceased F.Y. Edgeworth, whose disagreement with the Pearson and the biometricians had been about the goals and methods of statistical analysis. However, Pearson made it clear that it was a fondness made possible by victory. Specifically, Pearson told his audience that he had fond remembrances of Edgeworth despite the fact that Edgeworth had been a "Magister Obscurantissimus in his dialect,...blurring across our furrows...and his criticism failed as it had always failed, because he spoke not the language of the people."[98]

By 1929, the accomplishments of a second generation of biometricians could be celebrated as work that would

have made the early pioneers proud. For example, Pearson pointed to G.M. Morant's series of craniometric papers and good attendance in the "skullery"--part of the exhibit on the first floor of the Bartlett Building--and remarked that

> black as the prospects looked when Fawcett and Macdonell first applied biometric methods to the skull, the time is rapidly approaching when our craniometry will be the only craniometry. That is recognized in America, and will react on the Germans and the French, the former of whom are scientific naturalists, and the latter of whom accept nothing until late in the day they have found it out for themselves.[99]

The period of optimism about the future peaked in 1930, when Pearson announced that the "History of the Struggles" would be recounted in full in the final volume of his tribute to the Life, Letters and Labours of Francis Galton, and that the journal Biometrika was entering "its manhood in a stronger position than it has ever attained," due to the great interest in it in America and Japan, and the early signs of the possible conversion of the native populations of China and India into "Seekers of the Gospel."[100]

The period of post-war prosperity was, however, short-lived, and the revival of optimism in biometric discourse died with it. As forces converged to dismantle Pearson's research institute, biometric historical writing began to take on a bitter tone.[101] In historical accounts written by biometricians after

1930, the biometricians became heroes betrayed. They could only take solace in the fact that their historical legacy would be carried on elsewhere. "We cannot rival the laboratories of Berlin, Lund, and Munich," Pearson told his audience at the Galton Dinner in 1931, "we can only rejoice that Galton's new science--often under new national names--has spread over both old and new worlds."[102]

At the final Galton dinner in 1933, when the dismantling of the biometric research institute was inevitable, Pearson talked not of the history of biometry, but only of the future. "It will be for the future to estimate the value of the work these two united laboratories have done." Neither the College or the University had been able in the end to grasp Galton's idea, but it "doubtless will come to England some day again, as a foreign or American conception."[103]

## A Lesser Dream Fulfilled: the End of History

Three years after the final Galton dinner Karl Pearson died. In the December 1936 issue of Biometrika (Vol. 28, Parts 3 and 4), the first of a two-part memorial tribute to Karl Pearson appeared. It was the last piece of historical writing ever to appear in Biometrika. Preceding the memoir, entitled "Karl Pearson: An Appreciation of some Aspects of his Life and

Work," was a representation of Karl Pearson (see Figure 5). The image showed only the head and shoulders, almost in profile, representing Pearson as he looked in 1924 at the age of 67. The image was a pencil drawing, a faint evanescent image devoid of much detail. It was a representation of a fading memory.

The "appreciation" was the standard form of tribute to a great man of science upon his death. This particular one had a more personal tone and included more biographical elements, partly because its author was Egon Sharpe Pearson, Karl Pearson's son and successor as editor of Biometrika.

The younger Pearson identified the memoir as a "salute" offered at the "closing of an epoch" in the history of biometry.[104] His history of that epoch opened with a declaration of victory, a celebration of peace, and an order to disarm. Describing the development of the journal as an evolution, the new editor declared: "No spearhead of enthusiastic workers is now required to lead a fight for the recognition of the place of mathematics in the field of biology."[105] The new editor justified the publication of his historical tribute on the grounds that it would serve to guard against the danger implicit in victory--the danger of losing sight of the "simple yet bold objectives" that characterized biometry. In actuality, the history that

Figure 5. Sketch of Karl Pearson, circa 1924.
Published in Volume 28 of Biometrika, 1936.

Egon Pearson fashioned completely altered both the objectives and the identity of the biometricians.

The narrative that constituted the tribute was presented as a period piece. The life and work of Karl Pearson which characterized the previous epoch of biometry was depicted as belonging to another age. The author asked the modern biometrician to try to understand that Karl Pearson's biometry was created in, and infused with, the Victorian age, "a period when scientific thought, bursting free of the orthodox Christianity which it had learned in its youth, felt still a pressing need for some new philosophy of life to replace the old."[106]

Reading Egon Pearson's narrative, one can see the efforts of a son to understand his father. More to the point, however, one can read his attempts to distinguish between his father, and the "early biometry" which his father represented, and a different generation of biometrician. The identity of that new generation was that of "statistician." The very victory that Egon Pearson announced--the winning of the fight "for the recognition of the place of mathematics in the field of biology"--is a victory for a biometrician whose identity is that of professional statistician, not modernizing reformer of all knowledge about living things.

The younger Pearson's "salute" sought to put into context, that is, to isolate, explain, understand, bracket and deny the previous identity, and the practices that it inspired. Towards that end, Egon Pearson's history categorized his father's work into separate periods and presented them in a tale of personal development. The Karl Pearson of the 1880s was depicted as "a determined fighter against...misdirected authority," but also as a foolish young man, going through "a time of emotional development."[107] During this period, the young man was "clever, self-confident," and possessed "an unusually clear grasp of his subjects." But he was also one who, "when he disagrees, is often too eager to rush in and try to set the world aright."[108]

Taking the reader through a series of Karl Pearson's early writings, tracing the development of "that faith of a scientist which formed the basic philosophy of Pearson's life," the new editor of Biometrika distanced biometry from "early biometry's" inherent penchant for controversy. In the identity of the biometrician fashioned by Karl Pearson and his cohorts, controversy had been, however regretfully, a necessary component of biometry--something the reformer of scientific knowledge could not avoid. In the new identity that the younger Pearson fashioned, controversy

was not only regrettable, it was embarrassing. Specifically, Egon Pearson's historical judgment was that his father frequently succumbed to intellectual intolerance, a trait which could not be condoned. For posterity, he asked that the reader muster some sympathetic and understand that his father had been a "fighter roused in the heat of a battle for what he believed was the truth," and to remind themselves that trail-blazing scientists required supreme self-confidence.[109]

Reinforcing the feeling of distance between "modern biometry" and the biometry of the "earlier epoch," Egon Pearson's narrative passed quickly over his father's work on the "social and sex questions," characterizing them as problems of the past.[110] More emphasis was placed on Karl Pearson's life in the 1890s as the context for the founding and development of biometry. Summarizing his father's contributions to that development, the younger Pearson characterized the "main purpose" of his father's work during the early period as the "development and application of statistical methods for the study of problems of heredity and evolution."[111] From the point of view of the modern statistician, the new editor of Biometrika acknowledged that Karl Pearson's statistical work could be criticized for having been

> far too interested in pushing forward with the statistical examinations of problems of heredity to question very fully the theoretical possibilities of his results....Dangers that once understood seem obvious to later workers and openings which might have proved fruitful for new research are missed by the pioneers intent on blazing a trail into undiscovered lands.[112]

Such a criticism could be delivered confidently and gently from the new perspective of biometry as a mature science of statistical theory. "If we statisticians of today differ from the Pearson of 1900 [or]...of 1920, wrote Egon Pearson, "it is not primarily over a mathematical miscalculation, but from a change...in outlook between a younger and an older generation...which comes from historical development."[113]

The new historical narrative designated the year 1900 as marking the end of a phase in the history of biometry. It was the year of both the publication of the second edition of Karl Pearson's Grammar of Science which included two additional chapters on evolution, and of the rediscovery of the work of Gregor Mendel. The raucous disagreement between the biometricians and the Mendelians was the most notorious of the controversies from which the new biometricians sought to distance themselves. It was especially crucial to situate the incident in an acceptable historical perspective, given the ascendant position that Mendelism had come to occupy in biological circles, and the new position that Egon

Pearson was creating for biometry as a science devoted to the development of statistical theory that biologists (or any one else) could use as they saw fit. Accordingly, Egon Pearson's account of the period opened by clearly stating that

> between the Biometric and Mendelian schools there was nothing fundamentally incompatible; in pursuit of the same objective they followed different lines of approach, which were essentially complementary rather than antagonistic. With co-operation a big advance might have been made in the attack on the problems of evolution, and we might have seen many years earlier that combination of genetics, biometry and statistics, the value of which is being recognized more fully today.[114]

In passing historical judgment on the situation, the new editor of Biometrika displayed an even-handedness that the old editor never managed, but in the new analysis of the disagreement, the biometrician was consistently identified as a "mathematician," and juxtaposed to the "biologist." There was no reference to the biometrician as reformer. In the new narrative, the lack of co-operation between biometrician and Mendelian was explained in terms of three main causes: 1) the difference in outlook between the biologist and the mathematician, 2) an incompatibility of temperament among the leaders of the two schools, and 3) the early death of Weldon in 1906. In the new editor's judgment, the first cause was inevitable. To illustrate the point, he cited a passage from Karl Pearson's tribute to

Weldon to demonstrate that it was even hard to win Weldon over to biometry. Where the biologist was concerned the introduction of mathematics was "novel," and, therefore, bound to arouse suspicion. From this perspective, the suspicion of the biologists was "natural" and due to "that very human characteristic of rationalization, which classes as unsound and unnecessary that which cannot be readily understood." At the same time, the mathematician's reticence was attributed to the tendency of mathematicians to "underestimate the intellectual powers of the biologist."[115]

The new narrative also downplayed the role of controversy in the biometricians dealings with the Evolution Committee of the Royal Society. That story, which was such an integral part of Karl Pearson's historical narrative, was told in a single paragraph by his successor. Specifically, the younger Pearson merely noted that, in the context of the committee, statistical work proved to be controversial and was eventually abandoned in favor of research along Mendelian lines. In general, the increasingly controversial nature of biometric work in the first two decades of the 1900s was lamented, in the new narrative as having created an increasingly stormy atmosphere which instilled an attitude of suspicion in Karl Pearson. It was in this

atmosphere that Weldon uttered those fateful words--"the contention 'that numbers mean nothing and do not exist in Nature' is a serious thing which will have to be fought."--that gave birth to Biometrika.

In the new narrative, Weldon's death took on greater significance. Now it was depicted as having had a profound and regrettable effect on the relationship between genetics and biometry. From the new perspective, Weldon was seen as a bridge lost. He was a potential bridge between the biometricians and older biologists, and between "a younger generation of geneticists" who were schooled in the notion that "Mendel's conception differs fundamentally from that involved in the Law of Ancestral Heredity." In Weldon's absence, all involved (including Karl Pearson) missed what Pearson himself had said again and again, that Mendel's laws had an essentially statistical and descriptive character. The result was the production of a "myth regarding some essential error in the biometrician's approach has persisted to this day."[116] In the new era of biometry, it could be clearly seen that "no theory of inheritance could discredit certain established facts following from a statistical analysis of observational data."[117]

The rhetorical strategy of Egon Pearson's historical writing both distances the modern

biometrician from the excesses of the previous epoch, and defends biometry's reputation. From the perspective created by Egon Pearson's historical analysis, the biometry-Mendelism controversy could be seen as a regrettable, but understandable byproduct of the growth of a new science rather than as the result of some flaw in biometry itself. The historical perspective fashioned by his new narrative showed that biometry developed along a line parallel to the development of the physical sciences. Just as

> research into the behaviour of atoms in the mass--the statistical method of approach--had led to results of far-reaching significance in practical life long before any adequate model of the atom was available. So in the case to the theory of evolution the biometricians realized that the statistical line of attack, involving a study of populations under natural conditions, would lead to results of practical value which need not wait upon the construction and exhaustive testing of any detailed model giving an ordered picture of the process of individual inheritance.[118]

Early biometrician's interest in eugenics was to be understood in a similar way. Research into the inheritance of mental and psychological characters was depicted as following naturally from research into the inheritance of physical characters. The strong emphasis on race which made the work appear anachronistic to the modern reader was attributed to a pessimism that was a product of the historical context. The pessimism evident in Karl Pearson's

> discussion of national and social problems, which was undoubtedly associated with emotions roused by the South African War. That realization of failure, of national shortcomings and muddle-headedness which was brought home to the country in 1900 was certainly shared fully by Pearson.[119]

This process of disillusionment went hand in hand with a process of maturation which produced a Karl Pearson who, by 1905, pursued issues of eugenics out of a sense of duty that was quite understandable given the historical context. He was "no longer concerned with the faith of the scientist or the sociologist, but with the facts, and sometimes unpleasant facts, of experience which it is his duty as a scientist to set before the nation as a warning."[120]

Having contextualized those elements of early biometry that were alien to the modern biometrician, the new narrative returned to the story of the development of statistical theory and methods, taking the reader through a brief survey of Karl Pearson's application to statistical method during the period 1900-1906. The main point that the modern reader of Biometrika was to take from the story was that Karl Pearson's contributions "were almost invariably called forth by the need for solution of some practical problem."[121]

The second installment of the tribute to Karl Pearson appeared in Volume 29 of Biometrika, published in February of 1938. Bearing the same title as the first installment, Part II covered the years 1906-1936.

It opened with the assertion that 1906 was "a dividing point in Pearson's life," and that he was aware of it himself. The remainder of the tribute consisted of a periodization of Karl Pearson's life which constructed the years up until 1906 as period of youthful enthusiasm and creativity. The years between 1906 and 1914 were the years devoted to the foundation of a research institute, and the years from 1914 to Karl Pearson's death in 1936, defined a period in which he strove, sometimes successfully, sometimes not, to fulfill the dream of that institute.

The second installment completed and reinforced the work of the first. Its overall rhetorical effect was to create a present historical moment of the reader in 1938 that stood in contrast to a past time of the origins of biometry and its institutions. The metaphors that drove the narrative constituted the difference between the present and past identities as the natural result of progressive development in science.

In the final paragraph of the memoir, Egon Pearson entreated the modern biometrician to "leave" his father, and to be "confident that wherever the path of science may lead, Karl Pearson has contributed his full share of that pioneer work from which alone true progress can follow."[122] It was in that confidence that the biometrician could accept both his heritage and his

independence. The modern biometrician should always honor its dead, but he must also bury them.

## Notes

1. John Buchanan-Brown, et al., Le Mot Juste: A Dictionary of Classical and Foreign Words & Phrases (New York: Vintage Books, 1981), p. 35.

2. "Editorial," Biometrika I (1901): 1-6, p. 6.

3. The statue represented in the photograph was fashioned by H.R. Hope Pinker and presented to the University of Oxford by E.B. Poulton.

4. "Editorial," p. 1.

5. "Individual differences" was the term used by Darwin to denote the numerous small differences in character which occur among all members of a given population. Individual differences were understood to be a different type of variation from the less frequent, but more drastic type of variations, which were usually termed "sports of nature." The question of which type of variation was truly important for evolution was (and in some respects remains) one of the major topics of disagreement in debates over evolution. See Peter J. Bowler, Evolution: The History of an Idea (Berkeley: University of California Press, 1984).

6. "Editorial," p. 1.

7. Ibid., p. 1-2.

8. Ibid., p. 2.

9. See V.B. Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," Journal of the History of Biology 25 (1992): 1-65.

10. "Editorial," p. 5.

11. Ibid.

12. Ibid., p. 5.

13. Ibid.

14. Francis Galton, "Biometry," Biometrika I (1901): 7-10, p. 7.

15. Ibid.

16. Ibid., p. 8.

17. Jan Sapp has treated Galton's role in biometry in the context of a struggle for authority in the field of heredity. See Jan Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932: New Perspectives in the Rise of Genetics," Journal of the History of Biology 16 (1983): 311-342.

18. "Editorial," pp. 3-4.

19. Ibid., p. 4.

20. Ibid.

21. Ibid.

22. Ibid.

23. Galton, "Biometry," p. 8.

24. Ibid., p. 9.

25. Ibid.

26. Karl Pearson, "Walter Frank Raphael Weldon, 1860-1906," Biometrika 5 (1906-07): 1-52, p. 1.

27. Ibid., p. 16; and "Editorial," pp. 1-2.

28. Karl Pearson, "Weldon," p. 2.

29. Ibid.

30. Ibid.

31. Ibid.

32. Ibid.

33. Ibid., p. 35.

34. Ibid.

35. Ibid.

36. Ibid. Pearson omitted the name in the published quotation, merely as a matter of good form. One might be able to obtain it from the original letter.

37. Ibid.

38. Ibid.

39. Karl Pearson, "Discussion and Correspondence," Science 10 (1903). Offprint in Folder 348, Karl Pearson Papers, University College London Library, London.

40. Ibid.

41. Ibid.

42. Ibid.

43. Karl Pearson, "Weldon," p. 35. For the details of the biometry-Mendelism controversy, see Chapter 1.

44. Karl Pearson, "Weldon," p. 35.

45. Ibid.

46. Ibid., pp. 36-37.

47. Ibid., p. 37.

48. Ibid.

49. Ibid. See note 36.

50. Ibid.

51. Ibid., pp. 38-39.

52. Ibid., p. 39.

53. Karl Pearson, Report to the Worshipful Company of Drapers, 1903-1909, Folder 233, Karl Pearson Papers, University College London Library, London, p. 8.

54. "Obituary Notice of W.F.R.Weldon," Athenaeum 21 April (1906): 486. Copy in Folder 259, Envelope 6, Karl Pearson Papers, University College London Library, London.

55. H.H. Turner, "Letters to the Editor," Oxford Magazine, 31 (1906). Copy in Folder 259, Envelope 6, Karl Pearson Papers, University College London Library, London.

56. Karl Pearson, "Natural Selection," MS of a public lecture delivered at Newcastle, November 1903, Folder 61, Karl Pearson Papers, University College London Library, London, p. 1.

57. Ibid., p. 2.

58. Ibid., p. 4.

59. Ibid., p. 5.

60. Ibid., pp. 5-8.

61. Ibid., p. 10.

62. Ibid., p. 12.

63. Ibid., p. 13.

64. Ibid., pp. 14-15.

65. Ibid., pp. 16-17.

66. Ibid., pp. 17-25.

67. Ibid., p. 27.

68. Ibid., pp. 28-29.

69. Ibid., p. 29.

70. Ibid.

71. Karl Pearson, "The Origin of the White Man," MS of a lecture delivered at Cardiff and Newcastle, November 1903, Folder 61, Karl Pearson Papers, University College London Library, London, pp. 3-4.

72. Ibid., p. 5.

73. Ibid., p. 7.

74. Ibid., p. 7.

75. Ibid., pp. 11-16.

76. Ibid., p. 17.

77. Ibid., p. 21.

78. Ibid., p. 31.

79. Ibid., p. 39.

80. For the effect of the war on the biometric research program, see Chapter 2.

81. Karl Pearson, MS for his speech given at the occasion of the first annual Francis Galton Dinner, 17 January 1920, Folder 32, Karl Pearson Papers, University College London Library, London, p. 2.

82. Ibid., p. 3.

83. Ibid., p. 4.

84. Ibid.

85. Ibid., p. 5.

86. Karl Pearson, "History of the Biometric and Galton Laboratories," printed by University College London in conjunction with a fund raising effort, 12 March 1920, Folder 247, Karl Pearson Papers, University College London Library, London. p. 1.

87. Ibid.

88. Ibid.

89. Ibid.

90. Ibid.

91. Ibid.

92. Ibid.

93. Ibid., p. 2.

94. Ibid.

95. Ibid., p. 4.

96. Karl Pearson, MS of speech delivered on the occasion of the third annual Francis Galton Dinner, 17 January 1922, Folder 32, Karl Pearson Papers, University College London Library, London, p. 1. Pearson had initially written "modern science," and then crossed it out, replacing it with the broader term "modern knowledge."

97. Karl Pearson, MS of a speech delivered on the occasion of the seventh annual Francis Galton Dinner, 22

February 1926, Folder 32, Karl Pearson Papers, University College London Library, London, p. 6.

98. Ibid., p. 7. For Edgeworth's disagreement with the biometricians, see Chapter 1.

99. Karl Pearson, MS of speech given on the occasion of the tenth annual Francis Galton Dinner, 17 January 1929, Folder 32, Karl Pearson Papers, University College London Library, London, p. 3.

100. Karl Pearson, MS of a speech delivered on the occasion of the eleventh annual Francis Galton Dinner, 17 January 1930, Folder 32, Karl Pearson Papers, University College London Library, London, p. 1.

101. For the reasons behind the dismantling of Pearson's research institute, see Chapter 2.

102. Karl Pearson, MS of a speech delivered on the occasion of the 12th annual Francis Galton Dinner, 16 January 1931, Folder 32, Karl Pearson Papers, University College London Library, London, p. 6.

103. Karl Pearson, MS of a speech delivered on the occasion of the 14th and final Francis Galton Dinner, 17 January 1933, Folder 32, Karl Pearson Papers, University College London Library, London, p. 3.

104. E.S. Pearson, "Karl Pearson: An Appreciation of some Aspects of his Life and Work, Part I, 1857-1906," Biometrika 28 (1936): 193-257, p. 193.

105. Ibid.

106. Ibid., p. 194.

107. Ibid., pp. 196-197.

108. Ibid., p. 199.

109. Ibid., p. 204.

110. Ibid., p. 206.

111. Ibid., p. 220.

112. Ibid., p. 221.

113. Ibid., pp. 222-223.

114. Ibid., p. 227.

115. Ibid., p. 228.

116. Ibid.

117. Ibid., p. 242.

118. Ibid., p. 234.

119. Ibid., p. 237.

120. Ibid.

121. Ibid., p. 239.

122. E.S. Pearson, "Karl Pearson: An Appreciation of some Aspects of his Life and Work, Part II, 1906-1936," Biometrika 29 (1937-38): 161-248, p. 237.

# CHAPTER 4
# THE BIOGRAPHICAL GESTURE: THE LIFE OF WELDON

Volume 5 of Biometrika opened with a 52 page memoir, written by Karl Pearson, entitled "Walter Frank Raphael Weldon, 1860-1906." A salute to the co-founder and co-editor of the journal who died of pneumonia in April of 1906, the memoir is the most frequently cited primary source document for the early history of the biometric school. It is also a carefully crafted piece of historical writing. Chapter 3 analyzed this document, along with other historical memoirs published in Biometrika, to see how it contributed to the fashioning and maintenance of the collective identity of the biometricians, and of the historical contexts of particular debates. The goal of Chapter 4 is to examine how the narrative of Weldon's life, offered as a blueprint for the development of the perfect biometrician, was driven by a fundamental tension between the conflicting desires, one positivistic and the other Romantic, that characterized biometric rhetoric.

Pearson's salute to his dead friend opened with an "Apologia." The rest of it was divided into five

sections: Stock and Boyhood, Lehrjahre, Wanderjahre, London and the First Professoriate, 1891-1899, and Oxford and the Second Professoriate, 1900-1906. Together, these sections formed a narrative of the stages of development that led to the prototypical biometrician, Weldon.

In the "Apologia, " Pearson apologized for the biographical form of the memoir. The traditional way to honor a great man of science upon his death was to write an "appreciation" of his contributions to science. Weldon himself had written such an appreciation of his hero T.H. Huxley for the Dictionary of National Biography. Furthermore, to write an account of Weldon's life was, Pearson admitted, opposed to Weldon's preference, as Weldon thought all forms of personal biography to be pure vanity. In defense of his breach of etiquette, and of his decision to ignore his friend's preference, Pearson observed that anyone who had ever seen Weldon and Huxley together would "know that the Huxley of the appreciation was not all that Huxley meant to Weldon." Weldon had an "affectionate reverence" for Huxley that "did not spring from intellectual appreciation." Rather it sprung from "the influence of a strong character on a sympathetic character."[1] Weldon's relationship with his own students was described as being, like Huxley's before him, one that

inspired affection and admiration "not purely of a keen and strong intellect," but one that grew "from the strength of character, that subtle combination of force and tenderness, which led from respect for the master to keenest affection for the man."[2]

Pearson's telling of Weldon's life introduced several mentor/student characters, for whom science was "no less than theology or philosophy, [a] field of personal influence, for the creation of enthusiasm, and the establishment of ideals of self-discipline and self-development."[3] In short, Pearson's narrative made the proper pursuit of science something more than a mere intellectual activity. It made it a constitutive personal act which, if pursued in the proper spirit, could nourish and guide the development of the self.

In Pearson's narrative, however, the production of the proper kind of self was not guaranteed by the practice of science alone. On the contrary, Pearson warned that "science, like most forms of human activity, is occasionally liable to lose sight of its ultimate ends under a flood of controversy, the strugglings of personal ambition, or the fight for pecuniary rewards."[4] The danger was that the process of self development was easily derailed by petty ambitions which occasionally infected science. The solution was seen in the metaphorical equivalent of a vaccination. Pearson

believed that the practitioners of a particular science could be immunized from the infection through what he described as the "inculcation of high ideals among its younger votaries," through the personal interaction between the mentor and the disciple.[5] Weldon's untimely death abrogated the relationship between young biometricians and their schools' most charismatic mentor. Pearson's biographical account of Weldon's life was, in part, an attempt to preserve the lessons of that relationship in the collective historical memory of the group.

Pearson's articulation of the mentor and pupil relationship between Huxley and Weldon, and later between Weldon and his own students, spelled out the relationship that the developing biometrician was to have with the now textualized Weldon. One was not to worship the intellectual content of Weldon's scientific work, which was largely incomplete and might have to be abandoned in the future, but rather Weldon was to be worshipped in mutual affection, as the signature on the frontispiece photograph of him promised.[6] The Weldon that existed in Pearson's narrative was to be a master-guide for the journey of self development, effected through the practice of biometry, which led to the creation of a new kind of scientist and a new kind of "man."

Before giving the historical account of the process by which Weldon the man created Weldon the biometrician, Pearson articulated the biometric principles according to which any inquiry of human development ought to proceed.

> We see the environment--imposed and created...and we can, with accumulating experience, balance environment against heritage in the production of the highest type of scientific mind.[7]

This was an expression of the biometric ideal, with all its eugenic aspirations, which Pearson's account of Weldon's life embodied. The proper analysis of Weldon's inherited characteristics and environmental influences, balanced by a knowledge of the experiences that formed Weldon's life, would provide a blueprint for the proper development of a new kind of scientist--the biometrician.

## The Problem of Evolution in Man

In keeping with biometric principles of proper scientific investigation, Pearson's narrative began with a consideration of Weldon's "stock and boyhood." This section of the story focused on the striking resemblance of Raphael Weldon (as W.F.R. was known to his friends) to his father Walter Weldon. Of Raphael's paternal grandparents, Reuben Weldon and Esther Fowke, all that the reader was told was that they "belonged to the manufacturing middle class."[8] That descent from Weldon

stock was an asset to a would-be scientist was, however, established by the fact that Walter Weldon's life was recounted in the Dictionary of National Biography, the very source from which Francis Galton had drawn his sample for his pioneering biometric work Hereditary Genius.

The first detail of Walter Weldon's life specifically addressed in the narrative was his early career as a journalist, and particularly his editing , from 1860 to 1864, of the Register of Facts and Occurrences relating to Literature, the Sciences and the Arts. Pearson explained that this demonstrated that "while [Walter] Weldon's real name was to be made in science, his first interests were in literature and art."[9] The notion of a fully-rounded interest, which was shared by both the older and younger Weldon (and that was equally true of the eclectic Pearson), was emphasized as an important ingredient in the creation of the new scientist.

Walter Weldon's claim to DNB status was, however, due to his scientific interests and, most specifically to his contributions to the chemical processes of chlorine manufacture. While Pearson acknowledged this fact, he asserted that "for our present purposes, the main point is this: that Walter Weldon made his discovery while totally unacquainted with the methods of

quantitative chemical analysis and possibly because of this ignorance."[10] The point was so important, that Pearson completely dismissed Walter Weldon's own historical account of his discovery of the regeneration of manganese peroxide, and attributed the discovery to the elder Weldon's own innate ability to have keen insights in areas in which he was truly a novice. Pearson's narrative implied that this trait was heritable, connecting father to son by adding, in a lengthy footnote, several anecdotes crediting Raphael with the same ability.

The inheritance of Weldon's innate ability to contribute key insights to technical fields from an outsider or even novice perspective was important because the identity of the biometrician had been established elsewhere as that of a novice.[11] All biometricians were novices because the science was brand new. The novice identity served to blunt expert criticism from anyone who might wish to disagree with the conclusions reached in biometric papers. However, it also left the biometricians open to charges of dilettantish errors in their conclusions about essentially biological problems (to which the statistician Pearson was particularly vulnerable) or essentially statistical problems (to which Weldon was particularly vulnerable). Therefore it was essential to

establish in Weldon (and for biometry) an ability to make valuable and critical excursions into foreign fields.[12]

Similarities between Weldon and his father were also present in their deaths. Walter Weldon met an "early" death in September of 1885, two years after his son's marriage. The elder Weldon's career was, like his son's, cut off far before it was completed. But Pearson, in a sentence that applied equally well to both the father and the son (indeed, in Pearson's narrative, their stories became one), asserted that "his life had been lived to the full, each instant crowded with physical, intellectual, or emotional activity."[13] In the final analysis, Pearson suggested that the Weldons' lives were best understood in biometric fashion as a negative correlation between the intensity of life and life's longevity. Further, Pearson implied that Raphael Weldon had seemed to have grasped the point himself.

> There are men--not the least favoured of the Gods--who live so widely and so deeply, that they cannot live long. Discussions on the inheritance of longevity now come back to the memory, where Weldon referred to stocks of short-lived but intense life, and the personal experience and its molding effect on characters are now clear, where at the time the mind of the listeners ran solely on a correlation coefficient.[14]

In this passage Pearson, in the role of grieving friend, lamented having focused too much on the intellectual content of Weldon's work and exhorted the reader to

understand that the most important message of Weldon's life was a message of personal development. Despite full awareness of the costs, Weldon pushed on with life-consuming intensity towards his development into a biometrician.

Before leaving the subject of Walter Weldon's legacy, Pearson paused to mark one crucial distinction between Walter Weldon and his son. The father was given to mysticism. "Walter Weldon turned naturally to the mystical to satisfy his spiritual cravings; he was a Swedenborgian, and ipso facto a believer in intercourse with another world." In contrast, Raphael Weldon was "like his hero Huxley, a confirmed agnostic."[15] Pearson suggested that the elder Weldon's mysticism was natural, because he belonged to an older age, but in these matters the younger Weldon belonged to a different pedigree, a line of new modern men, self-created out of scientific endeavor. In this sense, Raphael Weldon was the progeny of Huxley. The effort to make this distinction reveals an important tension which shaped both biometric discourse and the biometric research program. It was a tension between the unrelenting positivism of Pearson's philosophy of science, and his essentially Romantic vision of biometry as breeding a new race of men who could make themselves and then reform the world in their own image. This tension was

to play a large role in the fashioning of both the biometrician's identity and the identity of the objects which they studied.

Upon the difference between father and son, Pearson's analysis of the Weldon stock turned to the maternal side. The only previous references to Raphael's mother in the memoir was a sentence informing the reader that "Walter Weldon married Anne Cotton at Belper, March 14, 1854."[16] Here, in the context of explaining Raphael's agnosticism, Pearson noted only that it might have been due to the mother's heritage. But the entire treatment of that heritage (there is no mention of the maternal grandparents) consisted of three sentences describing her as a devoted companion and a resourceful help-mate who exercised a disciplinarian influence on Raphael's environment. Other than that, she played no role in the narrative of Weldon's life.

From the brief consideration of maternal influence, the analysis moved to young Raphael's environment. Pearson reported that the history of Weldon's early childhood was very sketchy and that "of his childhood Weldon rarely spoke." There were only "occasional peeps of a solitary child who would retire for hours under the dining-room table with his Shakespeare, learning whole acts by heart."[17] The only detailed information related to Weldon's attendance, for not quite three

years beginning in 1873, at a school at Caversham run by a Mr. Watson. Mr. Watson's daughter Ellen had a brief career as a mathematician, and Pearson surmised that she may have "first stirred Weldon's mathematical tastes."[18] The school's pupils included many young men who later distinguished themselves in literature, science or art, but Pearson reported that one of its former pupils, probably E.B. Poulton, attributed this to the "special class from which it drew its chief material." The special class to which Poulton referred consisted of nonconformist families of some eminence.[19]

Perhaps the most important point in Pearson's account of Weldon's "Stock & Boyhood" came in a passage where Pearson asserted that even before 1870, the date of Weldon's first trip to the continent, "we find in the boy the father of the man. His great pleasure was to organise lectures for his children friends, and the adult population, if it could be procured."[20] In the boy we find the father of the man. That sentence contained the crux of the biometric view of the "problem of evolution in man" which was the topic of so many biometric investigations. The evolution of other life forms was a product--an effect--of natural selection acting on two components: environments and heritage. Human evolution, in the biometric view, had a third component--temperament. The need to take that third

component into account was the reason why human evolution required a eugenic science, and it was the reason why the practice of biometry, played out in microcosm in Weldon's life, was itself a eugenic practice. For temperament was the most controllable element in the process of human evolutionary development. In the biometric view, "man" could, if he had the necessary knowledge, seize control of his own evolutionary development and create himself. The biographical gesture in the historical writing of the biometricians, with all of its emphasis on fathers and sons, hereditarianism, and the importance of the novice, encapsulated the tension between the utilitarian positivism of late-nineteenth-century British culture and the Romantic dreams of a generation of its sons.

The only photo-icon, other than the frontispiece discussed in Chapter 3, included in the Weldon memoir was a photograph of "Raphael Weldon aged 10" (See Figure 6). The photograph provided an image of the boy--father of the man--seen from the bust up with a full head of hair parted neatly down the middle. The only remarkable feature was the eyes. They were large--comprehending. The overwhelming tone of the image was one of the slightly sad, but resolute, acceptance of the toll such a process of self-genesis would take.

Raphael Weldon aged 10

Figure 6. Photograph of Weldon, age 10. Published in Volume 5 of Biometrika, 1906-07.

Transmutation Begun: On the Path of Science

The next section of the memoir was entitled Lehrjahre, or learning years. In this section Pearson fashioned a story of Weldon's life during his early years as a student--first on the track of medical studies, then a brief stay as a student at the University of London (in 1876), and of his attendance at Daniel Oliver's general lectures on botany and Ray Lankester's on zoology. The story also contained information about Weldon's transfer, in the autumn of 1877, to King's College and his successful attempt at the Preliminary Scientific Examination of the London Medical Board. But all this was merely background to Pearson's conclusion that: "Weldon's earlier instinct to study biology was not substantially modified either by the choice of medicine as a profession or by the diversity of his London studies."[21]

The main drama of the short section on the Lehrjahre was provided by the episode of a "serious breakdown" which Weldon suffered in 1880. Weldon had moved his studies, in April of 1878, to St. John's College, Cambridge. Here, Pearson informed the reader, Weldon encountered "new and marked influences." These new influences, together with the stress brought on by overwork and a series of personal tragedies, led to the breakdown. In Pearson's narrative, the breakdown

represented what was in effect the "death" of the incipient (or larval) form of Weldon. In the few short years (and paragraphs) that encompassed the Lehrjahre, the Weldon of Pearson's narrative shed his old aspirations, his old environment, and his old emotional influences, the way a butterfly sheds the cocoon that characterizes the larval stage.

It was during this period at Cambridge that Weldon encountered the first of the men constituting the pedigree of the new scientifically oriented Weldon--Francis Balfour. According to Pearson, it was under Balfour's influence that "Weldon's thoughts turned more and more to zoology, and the medical profession became less and less attractive."[22] While working for Balfour 1879 to 1880, Weldon pushed himself relentlessly, working for his Tripos, beginning his own biological research on beetles, and serving as a teaching assistant for Balfour. At this point in the narrative, two seminal events occurred simultaneously: Weldon suffered the breakdown, and met Francis Galton for the first time. In Pearson's biographical narrative, Weldon's old identity began to crumble just as he met the man who would, in his new identity, become his father-figure.

The process of transmutation continued as Pearson brought the narrative into 1881, when Weldon, pressing on despite overwork, found himself physically and

mentally exhausted and unable to enter a competition for the college scholarship examinations. Through Balfour's influence, however, Weldon's ability was recognized and he received a scholarship.[23] The story continued with Weldon returning from a three month holiday in the south of France, and placing in the first class in the Natural Science Tripos. Further, Weldon accomplished this feat despite suffering a series of misfortunes, some of which occurred chronologically after the tripos, but are collapsed into a single "event" in the narrative. These misfortunes included the deaths, within the space of a few weeks, of his brother and his mother, the death of Balfour the following year, and the death of his father a few years later.

It was significant that all of Weldon's old identity fell away like a used up shell at precisely the moment in the narrative when Weldon's "real ability" as a man of science was appreciated. This textual moment served as the origin point for Weldon's journey towards self-development through science. Moreover, the first stage of this transmutation had sharpened Weldon's awareness of the cost of such a process of transmutation. In Pearson's words, it had left a man with "a certain tinge of melancholy, a doubt whether he too would live to finish his work, and a tendency to take the joy and fullness of life while it was

there."[24] The hero of Pearson's tale of heroic self-transmutation was a classic Romantic figure with a prescient awareness of a tragic fate that awaited him.

Transmutation Continued: On the Path to Biometry

Unfolding his narrative through the next three sections of the memoir, Pearson presented a tale of the new Weldon, emerging from the breakdown to re-create himself through the practice of science and, later, biometry. In the process, Pearson carefully constructed a historical context within which the reader was to understand and interpret Weldon's work.

Pearson presented the first twelve of Weldon's publications in the context of the *Wanderjahre*. As the title of the section suggested, these works were conceived during a period when Weldon was searching for himself and for direction. They were introduced in the narrative right after the death of Weldon's mentor Francis Balfour from an alpine accident in July of 1882. Of Balfour Pearson wrote: "The charm of Balfour's personality had aroused the affection of all who attended his classes, and had awakened a keen desire to follow, even if but a long way behind, in his footsteps."[25] The placement of these early works in their particular position in the narrative fashioned a context for Weldon's early publications which allowed them to be understood as the product of a personal

awakening precipitated by an affectionate mentor relationship with Balfour. They were the result also of the natural desire of the student to follow in his mentor's footsteps.

Pearson's story taught that such naive and blind adherence to the mentor's path was necessary, but ultimately had to be abandoned, lest the student follow the mentor over the precipice to the same "dead end." In the narrative, Pearson's Weldon faced and overcame that danger, initially following Balfour's metaphorical footsteps down a path of zoological study and into the footsteps of an even more heroic forerunner, Charles Darwin.

> In those days the stimulus given by Darwin's writings to the morphological and embryological researches was still the dominating factor amongst zoologists, and Weldon threw himself at first with ardor into the effort to advance our knowledge by morphological methods.[26]

Next, Pearson digressed from the narrative of Weldon's development to consider Weldon's power as a lecturer. Specifically, Pearson recounted a lecture that Weldon had given in 1885 to the Royal Institution on "Adaptation to surroundings as a factor in Animal Development." The digression was really a statement about the importance of environment as a factor in Weldon's development. "There are," Pearson wrote,

> two distinct sides to lecture work; the instruction of small or large classes of students and the public oration....In the former case, the

> eye must be kept on the average student, the lecturer must realize what the individual auditor is feeling....In this form Weldon was adept, it brought out all his force and enthusiasm as a teacher.[27]

To substantiate the point, Pearson offered an unidentified quotation from "a writer in the Times (April 18, 1906): Seldom is it given to a man to teach as Weldon taught. He awoke enthusiasm even in the dullest."[28] Public lecturing was, however, another matter. Here the "personal touch with individuals is not possible," and Weldon was not as successful. His "own intense thoroughness made him only too conscious when a portion of his audience was not following him; his highly nervous temperament made it a necessity that he should have a sympathetic grip on the individual."[29]

What was the point of this digression? In Pearson's overall narrative scheme, it illustrated the vulnerability of the developing scientist (Weldon) and of the developing science (biometry) to his (its) environment. In concluding the digression, Pearson asserted that "no man was more responsive to immediate environment than [Weldon] was. To do his best he needed a sympathetic environment."[30] Pearson further explicated the point by way of an analogy comparing Weldon's temperament to a torpedo-boat destroyer. After acknowledging that Weldon was a keen and ready debater, Pearson went on to note that , for Weldon,

> full action meant high pressure, it was a strain the less often repeated the better. A torpedo-boat destroyer is associated with a 26 knot speed, and such speed differentiates it from other vessels of war; but the less it is run at this rate, the longer undoubtedly it will last. Controversy was not an atmosphere in which Weldon rejoiced*; it came to him because he felt bound to criticize what he held to be error, because he must defend a friend, but it was running the destroyer at 26 knots![31]

By emphasizing the necessity of a sympathetic environment in which to carry out the search for truth, and constituting controversy as a needless infection which pollutes that environment, and which actually caused Weldon's "early death", Pearson produced a context in which all opposition to the biometric program could be understood to be poisoning the environment that made science possible. The asterisk in the torpedo-boat destroyer quotation directed the reader to a footnote in which Pearson underscored the point by contrasting the as yet unnamed breeders of controversy to Weldon who was so considerate of the feelings of others that he often hesitated to publish. "Yes, I know he is wrong," Weldon is quoted as saying of a fellow researcher's work, "but I don't want merely to controvert him, I want to get at the truth of these things for myself."[32]

The point made, Pearson resumed the narration of Weldon's development and introduced "the beginning of a new phase in Weldon's ideas." In this second phase of Weldon's transformation, Pearson characterized Weldon's

thoughts as turning from morphology to problems in variation and correlation. In a dramatic paragraph, Pearson related that

> The next year [1889] was to place in Weldon's hand a book--Francis Galton's Natural Inheritance, by which one avenue to the solution of such problems, one quantitative method of attacking organic correlation, was opened out to Weldon; and from this book as source sprang two of the friendships and the whole of the biometric movement.[33]

At this point in the story, Pearson presented yet another account of the genesis of the biometric school. This time, however, the genesis became an event in Weldon's personal transmutation. In this story, the source of both the biometric school and of Weldon's second-phase transmutation was a text: Francis Galton's Natural Inheritance. The conditions and characteristics of Weldon's earlier larval stage had, in the first part of the narrative, been described, analyzed, and compared to some of the characteristics of the fully developed biometrician. But here the narrative focused on one of the primary question upon which the new science of biometry was founded. It was a question that Weldon had posed in 1888: What is the relationship between the specific evolutionary characteristics of the two (larval and adult) forms, and what is the nature of the relationship between evolutionary transmutation and growth?

The question was appropriately raised in this section, because the second phase of Weldon's transmutation was not limited to a new set of problems, it also involved a self-induced change of environment and lifestyle. At the same time (1889) that Weldon changed his environment (moving to the laboratory at Plymouth), he began to find that the "dredging and collecting work" associated with his position at Cambridge "separated him from his books for half his time."[34] Accordingly, he applied for and received a year's leave from Cambridge to concentrate on his "book work." The new Weldon metamorphosed from old Cambridge naturalist, who dredged and collected in the dirt, to a new kind of life science researcher, who did his digging in books and laboratories like the one in Plymouth. Biometrika's reader-contributor was to understand that Weldon's publications during this period were produced in the context of that transition, and that the true significance of the period lay beyond those publications.[35]

The tale of the Wanderjahre continued with another digression. This one returned to the theme of the importance of well-rounded intellectual interest and talent, and concerned Weldon's "power with the pencil." Pearson described Weldon as "no mere draughtsman," and "an artist by instinct," and listed various examples to

demonstrate that Weldon exhibited "a real artistic power of creation."[36] Two examples of Weldon's work with the pencil were reproduced to underscore the point: a drawing of a snail and one of an old man's face (see Figure 7). The juxtaposition illustrated the dual subjects of Pearson's biographical narrative: the progressive evolutionary development of the character of all living things, and of "man" through the practice of science. This dual image was invoked repeatedly throughout the rest of the memoir.

Pearson concluded the tale of Weldon's Wanderjahre with Weldon's succession of Ray Lankester in the Jodrell Professorship at University College, London in December of 1890. Pearson noted that, in June of that year, Weldon had been elected a Fellow of the Royal Society largely on the basis of his first two biometric papers.[37] But Pearson postponed the consideration of those papers until the next section of the narrative. The biometric papers did not belong to Pearson's narrative of the Wanderjahre, because they were to be understood as the product of a mature biometrician. In Pearson's narrative, chronology was made subservient to symbolic importance.

The Wanderjahre were summed up as years of intense activity in which Weldon was "teaching many things, studying many things, and planning many things." They

(*a*) "L'Apparition: Le Café Orleans."

(*b*) H. Hortensis, from a letter.

Figure 7. Examples from Weldon's sketch-book. Published in Volume 5 of Biometrika, 1906-07.

were not wasted years, but rather years in which Weldon had perfected his linguistic powers, which in time "opened to him also those stores of literature, which appealed so strongly to his artistic temperament. From the medieval epics to Balzac he was equally at home in French literature; and the Italian historians were read and carefully abstracted."[38]

Only from the convergence of such a varied background and array of talents could the "new man of science" emerge. A new man whose will-to-know could not be contained by traditional sciences like morphology and embryology. Only the life of such a man could give birth to the new science of biometry:

> His remarkable thoroughness in science re-appeared as a form of scholarly instinct when he approached history and literature, and the present writer remembers Weldon's keen pleasure and exactitude in following up more than one historical enquiry. His delight in knowing spread far beyond the limits of natural science.[39]

Weldon's earlier scientific pursuits, morphology and embryology, were compared in Pearson's account of Weldon's life, to "draughting" which was accurate in detail, but lifeless. Weldon, in his new incarnation, possessed the artistic powers of creation and was compelled to move beyond these earlier pursuits. Similarly, the traditional "appreciation" of a scientific career was also accurate in detail, but lifeless. In contrast, Pearson's biographical account

was designed to possess creative power--the power to generate progeny, specifically a generation of new men of science--the biometricians.[40]

Becoming a Biometrician: the Elimination of the Self

The fifth section of Pearson's account of Weldon's life, entitled "London and the First Professoriate, 1891-1899," opened with yet another summary of the "transition" of the Wanderjahre. Here Pearson characterized Weldon's transition as one from a young man gripped, like most young men of the day, by an overwhelming enthusiasm for the Darwinian theory of evolution, which "threw open to him, as to them, a wholly new view of life with its possibility of seeing things as a connected whole," to a man who realized that "the great scheme of Darwin was only a working hypothesis, and that it was left to his disciples to complete the proofs, of which the master had only sketched an outline."[41]

In this characterization, Pearson drew together many of the themes and images of the previous sections. The connected whole of the Darwinian view of life was opposed to the piece-meal reconstructions of the morphological and embryological views. The image of Darwin as master was re-affirmed in a slightly different way, as the words "proofs" and "sketched" evoked both the images of the statistical and genealogical

approaches of biometry, and the images of the creative artistic instinct which allowed the maturing biometrician to go beyond the master.

Pearson's account of the Wanderjahre encouraged the reader to view Weldon's early embracing of morphological and embryological methods and concerns to have been natural, harmless, and inevitably brief. It also conditioned the reader to accept as natural the fact that Weldon would, and did, come "to the conclusion that no great progress could be attained by the old methods." From the transition period that was the Wanderjahre, a Weldon emerged whose "thoughts were turning on the distribution of variation and the correlation of organic characters"--a Weldon who was "being led in the direction of statistical inquiry."[42]

Here again, Pearson's biographical account of Weldon's personal development continued to be his metaphor for the proper development of science itself. The natural progression of the development of Weldon's thoughts, ideas, and questions was to be seen as a product of the development of Weldon himself. Those who would ignore that order and try to sum up Weldon's ideas without knowledge of the progression of his life, would miss all the valuable lessons, and would not understand their development. Similarly, those who would try to answer the last question in the sequence of questions

concerning evolution, that is, those who would try to solve the mystery of heredity while ignoring the process of evolution, would not fully understand the question whose answer they sought.

The narrative of Weldon's first professoriate began a year earlier than its title suggested, as Pearson reached back to 1890, when Weldon was working in Plymouth, to include the work that produced the first two biometric papers. Six paragraphs were devoted to this year and the two papers written in it are characterized as having been "epoch-making in the history of the science, afterwards called biometry."[43] Pearson explained and defended the characterization at length, addressing several factors necessary for the validation of the claim, and reiterating the "suggestive" value of Weldon's early work. First Pearson noted that the Weldon of 1890, like Darwin before him, possessed a very limited mathematical knowledge. What Weldon did possess was the force of character that compelled him to increase his mathematical knowledge. This effort led him, not to elementary textbooks, but with characteristic thoroughness, to the great French writers on the calculus of probability.[44]

The story of the 1890 papers served as an introduction to several episodes in which collaboration

was the connecting theme. The first collaboration involved Weldon with Francis Galton who, as referee of Weldon's paper, found it intriguing but in need of extensive remodelling in both its statistical treatments and its conclusions.[45] The rest of the series of collaborations involved Weldon with Pearson. More specifically, Pearson introduced a series of stories about his own collaborations with Weldon by asserting that Weldon's mathematical difficulties were balanced by a different talent: "a touch with observation and experiment rare in mathematicians."[46]

Biometry, the reader was to understand, was born out of the combination of Weldon's touch with observation and experiment and Pearson's touch for higher mathematics. Pearson's narrative made the point in the context of the developing personal relationship between Weldon and Pearson, telling the story of the "biometric lunches."

> Weldon and the present writer both lectured from 1 to 2, and the lunch table, between 12 to 1, was the scene of many a friendly battle, the time when problems were suggested, solutions brought, and even worked out on the back of the <u>menu</u> or by aid of pellets of bread.[47]

The above passage is quoted in almost all of the histories of biometry and is usually presented as the prime example of the kind of collaboration that gave birth to the biometric school. But in Pearson's

narrative the story moved beyond collaboration to the merging of identities.

> It is difficult now, after fifteen years of common work and continuous interchange of ideas, to distinguish where one or other idea had its source, but of this the writer feels sure, that his earliest contributions to biometry were the direct results of Weldon's suggestions and would never have been carried out without his inspiration and enthusiasm. Both were drawn independently by Galton's Natural Inheritance to these problems. But the papers on variation and correlation in shrimps--which in rough outlines are types of all later biometry--were published before their friendship had begun.[48]

On the surface the paragraph simply credited Weldon with the creation of biometric principles, and established Weldon's first two papers as models of biometry which could be emulated by future generations. The deeper effect of the paragraph, however, was to blur the lines between Weldon and Pearson as the origin source of most early biometry. In Pearson's narrative, Weldon and Pearson have become one. The story of Pearson's life, was implicitly present, as the narrative became a story of two developing wanderers drawn separately to Galton, and giving birth to biometry through their intercourse at the famous biometric lunches. Their union produced progeny, the biometricians, who inherited both the traits of the traditional man of science, a touch for observation and experiment, and the additional trait of the new man of science, an aptitude for higher mathematics.

The same message of unity of identity was conveyed in the third collaboration tale. Set in the years 1891 and 1892, the tale once again opened with the presentation of another "epoch-making" effort by Weldon.[49] Here again, biometry was a result of Weldon and Pearson's personal union, as Weldon's lack of mathematical expertise led Pearson to bring forth a formative contribution to biometry. Specifically, Weldon confirmed in the results achieved with shore crabs at the Plymouth lab, the same type of results that he had earlier reached for the common shrimp: "The distributions of characters are closely Gaussian with the exception of the relative frontal breadth."[50] The exception--the problem of the frontal breadth--led to Pearson's first paper in the series entitled Contributions to the Mathematical Theory of Evolution. Again following the form of the previous stories, Pearson pointed out that Weldon's unpublished memoirs reveal that Weldon had reached a "moderately accurate solution by trial and error" before proposing the problem to Pearson, but made no reference to this fact in the published paper. The story again illustrated the model biometric collaboration in which the strengths and weaknesses in Weldon's ability spawn a contribution from another. In such a collaboration, the differentiation

of individual accomplishment was not only unnecessary, but impossible.

The stories of the first professoriate all shared a neo-Romantic quality which emphasized the heroic struggle to create a "new man", while they simultaneously emphasized the theme of blurring individual identities and self-elimination. These stories communicated the essential tension in the development of the biometrician--the goal was to develop a new breed of scientific man by submerging the individual identity, with its accompanying petty ambitions and vanities, into the collective identity of biometrician. The self-development that created the biometrician eventually led to self-elimination.[51]

The fourth collaboration tale differed from the others. It lacked both the neo-Romantic quality and the immersion of self. Where the first three were happy stories about the kind of collaboration that becomes possible in the world of pure science practiced by biometricians, the fourth story served as an example of the chaos that ensues when individual egos are given free reign in the sordid world of academia. Specifically, the story was about an effort, underway since 1884, to reform the University of London. In it, Pearson told of a bureaucratically hampered and unclear

debate in which "scarcely anyone had a notion of what a real university must connote."[52]

The story unfolded in an ominous air of mystery as Pearson spun a shadowy narrative in which a little group of determined reformers battled College authorities and mysterious "outside influences," while braving rumors of dismissal from their department chairs.[53] The group's efforts met with early success, but things began to turn sour when they "considered that we ought to have a leader of great name, and we asked [T.H.] Huxley to be president." Huxley accepted, but his views diverged from the original vision of the group. Huxley sought an immediate compromise, whereas the group had envisioned "a long campaign to impress the powers that be with true notions of academic life."[54] Once individual egos were introduced, the group lost its coherence. Pearson felt compelled to go to the press to explain how and why the original vision of the group was being subverted, and Weldon felt compelled to defend his hero Huxley.

Pearson summarized the episode by referring to the qualities possessed by Weldon--his loyalty to his hero, his reserve and ability to separate disagreements in one sphere from the working relationships in another, and his deeper than average feelings. But the forceful moral of the tale came in its illustration of the ways in which petty personal and political differences

inherent in controversies could scuttle true collaborations. That theme was made even clearer in an analogous episode which followed it in the narrative.

The second episode was a about the "Royal Society Evolution (Animals and Plants) Committee." Pearson began his telling of the story of the Evolution Committee with a coyness that preceded and identified all stories of controversy in biometric discourse. He opened by asserting that "it is a little difficult to give the full history at present," but that some attempt must be made. Pearson also left no room for the reader to doubt that the story would be one of failure, as he prefaced its telling with a paragraph evaluating the general value of committees in science:

> If used as instruments of research, the work done is too often a compromise between different methods and divergent personalities; if merely administrative they are successful or not, according to the width of view of some dominating temperament. If run in the interests of one school, still more of one individual, a committee may no doubt do good work, but it is likely, at the same time, seriously to damage the reputation of any larger body in whose name it works, by too markedly connecting it with one aspect of a problem or one side of an unsettled controversy.[55]

In Pearson's account, the Evolution Committee developed out of an informal discussion between Weldon, Galton, and R. Meldola at the Savile Club on December 9th, 1893. Out of that meeting came a formal proposal to the Royal Society that a committee be established and

funded "for the purpose of conducting Statistical Enquiry into the Variability of Organisms." The proposal listed as desirable members the three originators plus F. Darwin, A. MacAlister, E.B. Poulton, and a statistician to be named later.[56]

The Royal Society constituted the committee, which first met on January 25, 1894, with the six named members and with Galton serving as chairman and Weldon as secretary. The committee was officially entitled the "Committee for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals," and Pearson noted that "the use of the words statistical and measurable, somewhat narrowly, but accurately defined the proposed researches of the Committee."[57]

The "early biometric period" of the Committee came to an end with a substantial increase in the committee's scope and membership in February of 1897. Its scope was increased by adding to its original objectives the "accurate investigation of variation, heredity, selection, and other phenomena relating to evolution." Its membership increased with the addition of William Bateson, S.H. Burbury, F.D. Godman, W. Heape, E.R. Lankester, M. Masters, Karl Pearson, O. Salvin, and Thistleton-Dyer.[58]

There were, Pearson related, "great hopes of achievement" for the newly constituted committee.

> Francis Galton struggled bravely for a great idea....He strove to make two schools, widely diverse in method and aim, understand each other....But it was not to be. The members were pulling in opposite directions, there was too much friction, and too little compromise. A false antithesis was raised between what was termed "natural history" and any sort of statistical inquiry leading to mathematical results. The biometric members ceased to attend regularly and finally resigned towards the end of 1899.[59]

The passage quoted above contained the first reference to a competing (if nameless) school, and to a false antithesis between natural history and biometry. Pearson concluded by informing the reader that

> from the beginning to end the Committee has, in the opinion of the present writer, been a mistake; not only because at first it distinctly forced the pace of Weldon's work, but because experience shows that such a committee can only work effectually in the interests of one school of ideas, and this, whatever safeguards may be taken, has at least the appearance of destroying the impartiality of the parent body, a matter of grave importance.[60]

Pearson described the effect of the Committee on Weldon's work during this period as generally negative. Weldon's research program was troubled by being ahead of its time, and its suggestiveness was lost in a sea of controversy. Rather than produce progeny, the intellectual seed of Weldon could find no purchase in a scientific womb that was crowded with individual egos.

Still, even in a hostile environment Weldon was able to carry out the experiments and excessive labor that provided a "proof" of the effectiveness of natural selection. Returning to the theme of the value of

Weldon's artistic instinct and literary training to his scientific expression, Pearson described the papers Weldon produced in this period as "models of clear exposition." But this, Pearson stressed was the result of not only of a gift, but of much labor--constant re-writing until the proper form was reached. To Weldon, "a paper was a literary whole, which had not only to convey new facts, but to play its part on the scientific stage." His ability to choose the proper form and envision the whole was "comparable with Weldon's sense of sound, with his extraordinary gift of appreciating and reproducing the exact intonation of a foreign tongue. Both were the result of observation and experiment--not manifest in the final product--guided by an artistic sense."[61]

Weldon's best biometric papers were, in Pearson's narrative, the result of the cultivation, in a man and in a science, of a multitude of talents and affinities, and the thorough and hard labor of observation and experiment. When these ingredients came together--having been allowed to mature in an environment free of all of the strife caused by the presence of individual egos--a virtuoso performance was given on the stage of science. Weldon's overall performance during the period of the first professoriate was greatly hindered by an unruly and hostile mob of individuals.

Both of Pearson's tales about committees--the reform committee and the evolution committee--ended in failure. They served to illustrate the sharp contrast between the type of unspoiled collaboration between biometricians in the world of pure science, and the ruinous effects of committees in the impure world of personal and political ambition. To the biometrician, committee work was an exercise in futility. Its product was, at best, a negotiated compromise. The work, the biometry, the science that emerged from the personal relationship between Weldon and Pearson was more than a collaboration. It was of a single piece--a hybrid. To become the kind of scientist who could produce great work, the biometrician had to be a willing and loving apprentice, and should not desire the school of biometry to operate like a committee. Rather, the biometrician had to strive to eliminate the self from, and through, the practice of biometry. In short, the repeated lesson of Pearson's biographical account of Weldon's life was that the biometrician should strive for an almost Romantic notion of "oneness," while simultaneously erasing individuality from the one truly pure practice of science.

## Finding the Way

The final section of Pearson's account of Weldon's life was entitled "Second Professoriate, 1900--1906."

It covered Weldon`s move to Oxford and told the story of the final phase of Weldon's transmutation. It also offered analysis that unpacked for the would-be biometrician the lessons embedded in Weldon's life.

Weldon's friends had high hopes for his move to Oxford. They hoped life at Oxford would offer Weldon a respite from the pressures of a too full life. The pace of life was certainly slower at Oxford, but Pearson recounted that Weldon had some regrets over the move. The "Oxford boys" were the antithesis of the kind of student that the co-founder(s) of biometry wished to cultivate.

> [Weldon] would speak with great affection of dear old Gower Street, where everybody was working and everybody wanted to work--and he would be vexed that so many of these nice Oxford boys had no res angusta domi to force them from the river and the playing field into the laboratory and the lecture room. "They are so nice, they come to my lectures because they think it would be rude to leave me alone." The lad who would not make a sacrifice to his love of science--accept an Asiatic appointment of the merest bread and butter value, or take passage in a tramp steamer to collect in South America--was anathema to him. He wanted everywhere an infant Huxley.[62]

Pearson, however, intimated that his friend was merely being impatient. While the role of the mentor in the production of biometric offspring was crucial, such offspring could only be produced over time through a personal evolution. Weldon, in his impatience, had lost sight of the fact that it was "largely his own personality which had created the band of earnest

workers round him in London, and that with time it would be effective in more conservative Oxford."[63]

The second point that Pearson made concerning Weldon's move to the new environs of Oxford served to answer a charge, said to have been levelled by "the competing school" on the Evolution Committee, that biometry was the antithesis of natural history. First Pearson established that "Weldon's taste, his whole emotional nature, made him essentially a field naturalist."[64] As evidence, Pearson reproduced part of a letter sent to him by Weldon which recounted the joy Weldon felt being outdoors again. Parts of the passage could indeed have come from any naturalist's notebook, as this fragment shows:

> Nevertheless, my head is so full of chalk-downs and clouds, and things, I can't write biometry tonight. Always, when I have been with the country, the feeling breaks out that the other folk have the best of it. The other way you live with the country and become part of it; and you dredge, or fish, or shoot something wonderful, and you describe it, and everyone sees that it is wonderful, and you all enjoy the wonder.[65]

Next Pearson explained the reasons that Weldon was drawn out of the idyllic countryside and into the dark cramped calculating rooms:

> It was no innate taste for figures or symbols, no pleasure in arm-chair work, which drew him to statistical research. Nor was it the influence of any personality. On the contrary, he was impelled to it by the feeling that no further progress with Darwinism could be made until demonstration from the statistical side was forthcoming....He was not drawn into actuarial work by his sympathies or his

> friendships, he was driven into it by the looseness he discerned in much biological reasoning.[66]

Weldon's development as biometrician was the product solely of a personal evolution which reflected the progressive evolution of science itself. Weldon found himself on the path of biometry, because other avenues proved to be evolutionary dead-ends.

Once on the path, the development of Weldon's character and of the methods of biometry proceeded side by side. Pearson illustrated the biometric approach by examining Weldon's studies with land snails. The problem was to understand the "meaning of the slight but perfectly sensible differences in type to be found in shells from adjacent valleys, or even from different heights of the same mountain."[67] The "usual manner" consisted of patient collecting (usually over Christmas vacations) of all manner of variations and bringing them back for measurement. But not just superficial measurement: "No rapidly made measurement on the outside of the shell would satisfy Weldon; the shell must be carefully ground down through the axis, and measurements made on the section thus exposed."[68] The absolutely thorough penetration of nature was the biometric way:

> The Sicilian snails remain as an indication of the way--the path of absolute thoroughness--the master would have us follow.[69]

In a strong paragraph, Pearson spelled out for the reader the important legacy of Weldon's work during this period. That legacy consisted of the identification of "the problem which everyone interested in Darwinism desires to see answered," namely: "Does selection take place between birth and the adult or reproductive stage?" To answer such a question, the scientific investigator had to "compare the characters of the organisms at the same stage of growth, for these characters were modified by growth." Weldon showed how this could be done exploiting the "brilliant idea that the snail carries with it practically a record of its youth." The snails shell provided a record of its growth--a history of its development. "If the wear and tear of the outside of the shell to some extent confuses the record there, a carefully ground axial section will reveal by the lower whorls the infancy of the organism."[70]

In the same way that a superficial examination of the surface of the snail shell was insufficient, examination of Weldon's publications was no measure of his contribution to biometry and science in general. Weldon did science for himself at all times.

> He pursued science...for sheer love of it, and he would have continued to do so had he been Alexander Selkirk on the island with no opportunity for publication and nobody to communicate his results to. He never slackened in the total energy he gave to scientific work, but

> having satisfied himself in one quest, he did not stay to fill in the page for others to read.[71]

Weldon's self-less dedication to the furthering of knowledge was what made his contribution to science impossible to determine by a superficial look at his publications. Weldon "went forward, regardless of publication and finality of form." His "true function" was not to tend to the mundane details of publication, but rather to be "directing and inspiring a school, which will be trained by completing the work an carrying out the suggestion of its master."[72]

In his failure to realize his stated goals, Weldon had provided "the way--the path" to true knowledge about Darwin's theory.

> Hence the days given to experimental grinding, the training in manipulation and the final success, and then the steady work, grinding and measuring a few specimens a day, till the necessary hundreds were put together; the laborious calculations not in the least indicated in the papers--the arithmetical slips with bad days of depression, and the completed result: the illustration of how shells may be used--by those who will give the needful toil--to test the truth of the Darwinian theory.[73]

Clues to the history of the development of a pedigree were hidden in the record of an individual's own development, which it carried within itself. Again the reader was forced to consider the juxtaposed images of Weldon's sketchings of the snail and the man. Just as the snail carried a hidden record of its development in its shell, waiting to be carefully explicated by the

patient and sympathetic investigator; so did Raphael Weldon offer a record of his development--of the development of the prototypical biometrician--in his life, waiting to be explicated by the patient and sympathetic biographer. And just as wear and tear to the outer area of the shell could sometimes obscure and mislead the investigator, requiring him or her to patiently grind out and examine the interior; so to did the outer record of the Weldon's life in science--his published work--mislead. It was the inner life that instructed and illuminated.

In Pearson's narrative, Weldon became, in the final phase of his transmutation, the "master," supplanting Darwin as the illuminator of "the way--the path." The path was followed only by developing the sensitivity and patience to proceed with absolute thoroughness. For Pearson, this was the crucial fact that scientists had to face up to, for all else was superficiality. Pearson concluded the lesson by juxtaposing the fictional murmerings of a superficial critic: "Life is not long enough for biometry," to the poetic reply of "the man of deeper insight:"

> That low man goes on adding one to one,
> His hundred's soon hit:
> This high man, aiming at a million,
> Misses an unit.[74]

Weldon's life provided all the example that was needed for those who wished to follow the path that he

had blazed. But how was one to begin? A metaphor for the proper conduct of "the Brethren" at the beginning of their journey was given in a series of otherwise curious passages--passages that brought the reader a glimpse of a biometrician and his dog.

> Where are you going at Easter? Stone wall country is very good, and if you find a place with delightful old stone villages and pretty churches, Draba verna will be there! Come into this region, with the bike, and learn to know and love the dear Dog.*[75]

The asterisk directed the reader to a footnote concerning the dear Dog:

> The great Borzoi Sandro, henceforward to be a marked feature of the Weldon household, at home and away from home. Sandro pursuing sheep over the Yorkshire moors, Sandro pursuing game in the Buckingham beechwoods, Weldon pursuing Sandro with every tone of affectionate persuasion, on the track the stacked cycles and the co-editor pursuing the deserted biometric problems in solitude, Weldon returning with the unchastised dog, after any interval of from 10 to 40 minutes, the chase being fully completed, the apologies for the Borzoi, his sustentation on chocolate and the human need for cigarettes, the return to the cycles, to the experiment that was to be crucial, to the colour and the sunset, these are all of the memories, the like of which others will have shared, which helped to form the atmosphere about the man. Sandro achieved his purpose, he kept his master out in the air--such wolf hounds can follow a cycle for miles--and to exercise him was held up as moral duty [my emphasis]. But his limited intelligence led to his own disablement and he had to become partaker only in biometric "at homes." For two years, however, he was a great feature of our joint expeditions.[76]

The major notes of the by now familiar tune ring clear: "affectionate persuasion," "the atmosphere about the man," "moral duty." The path--the way--to personal

transmutation through biometry was found and the journey was sustained by these things. The biometrician encountered the path and entered into the way, following it with the single-mindedness of the affectionate (even loving) novice--whose model for the early stages of the journey was Sandro. The analogy was again invoked several pages later:

> There was absolute certainty that if the problem was at all an exciting one, Weldon would leave his scent and follow the new trail with his whole keenness and at full speed. All else would be put on one side, and he could only be recalled to natural history or biometrics by an appeal to conscience. Like Sandro, the chase must be complete before he returned to the humdrum trot behind a cycle on the highway.[77]

One followed the path dutifully through the steady development of three characteristics: affection, moral force, and keen intelligence. The original Sandro, alas, possessed only two of the three. From the proper perspective, from a view from the path, the moral superiority of the biometric way was clearly seen. Only from that perspective could the full value of Weldon's life and work to the development of science be understood.

An Easter theme served as the metaphor which finally allowed Pearson's narrative of Weldon's life to come to its inevitable completion. Walter Frank Raphael Weldon died on Good Friday. If his disciples were to find salvation by following him down the path along

which biometry and science might progress, they would, Pearson challenged, have to fight. They would have to fight, "not for this theory or that, but for a new method and a greater standard of logical exactness in the science of life."[78]

Pearson's concluded with a warning to biometry's enemies and an exhortation to its Brethren in whose hands Weldon's legacy rested. The warning to the enemy emphasized that their battle was already lost, for biometry has shifted the criteria for success. It has changed the standard by which something can be could be called scientific knowledge about life, and only the biometry could meet this standard.

> To those who condemn [biometry] out of hand, or emphasise its slightest slip, we can boldly reply, You simply cannot judge, for you have not the requisite knowledge.[79]

The exhortation to the biometricians rested on the same notion. They should follow Weldon, because he was and "will remain as the first biologist who, able to make his name by following the old tracks, chose to strike out on a new path--one which carried him far away from his earlier colleagues."[80] This was the message that the disciples might read from Weldon's life: If they followed the path faithfully, then when the time came when the question of "whether Darwinism is the basal rule of life or merely a golden dream which has

led us onwards to greater intellectual insight," the biometrician would be the one who answered.[81]

With that said, Weldon could now be carried off, on the shoulders of his affectionate warriors.

> Step to a tune, square the chests, erect each head,
> 'Ware the beholders!
> This is our master, famous, calm and dead,
> Borne on our shoulders.[82]

## The Equation of Objectivity

The authors of biometric discourse identified biometry with reform. Biometry was to be a new kind of science, practiced by a new kind of man, which would provide the knowledge upon which modern society could be rationalized. Confirmed Darwinists, the biometricians saw "man" and his societies as the product of evolutionary struggle, produced by the effects of natural selection upon <u>three</u> factors. Where all life forms and their behaviors were produced by natural selection acting on some mixture of heredity and environment, "man" possessed a third relevant factor--temperament. The preeminence of "man," and of western European societies in particular, that the biometricians perceived, they attributed to the presence of an evolutionarily advantageous temperament. "In the struggle for existence," wrote Karl Pearson, "man has won his dictatorship over other forms of life by his power of foreseeing the effects which flow from

antecedent causes--not only by his memory of past experience, but by his power of generalizing experience in scientific statements."[83] The practice of biometry would not only produce the knowledge about heredity and environment necessary to fashion laws and regulations to properly assist the continued evolution of "man" and his society, it would also produce the proper temperament--the "habit of science."

To bring about social reform through biometry, the biometricians had to be able to speak with the authority of science about the nature and relationship of the three components of human evolution. In the context of late-nineteenth and early-twentieth-century Britain, that meant demonstrating the total objectivity demanded by realist and positivist modes of discourse. This imperative required the identity of the biometrician to be infused with a central tension between the essentially Romantic desire for self-generation and the positivistic desire for self-elimination.

This chapter examined the model that Karl Pearson provided for the evolutionary development of a biometrician--his account of the life of the co-founder of biometry, W.F.R. Weldon. In his telling of a Weldon's life, Pearson outlined the evolutionary development of the prototypical biometrician. Beginning as a boy with an inherited ability for innovation,

developing--sometimes aided by an advantageous environment, sometimes hindered by the lack of it--a mind for science, and metamorphosing finally into a biometrician through the complete elimination of the individual self whose individual ego was lost in the collective endeavor of biometry.

The destruction of the individual self was a crucial stage in the development of the biometrician because it was what guaranteed objectivity. Pearson argued that the element of science that set it apart from other belief systems and, therefore, made it deserving of complete authority in the matter of social reform, was its objectivity.

"A philosophic or religious formula," Pearson wrote,

> really appeals to the individual temperament, and is accepted or rejected according to the emotional sympathies of the individual. On the other hand, a formula, like that which Newton propounded for the motion of the planetary system, will be accepted by any rational mind which has once understood its terms and clearly analyzed the facts it resumes.[84]

A mathematical formula was produced by methods which eliminated "individual bias." Extrapolating from that point, Pearson argued that methods like

> the classification of facts and the formation of absolute judgments upon the basis of this classification--judgments independent of the idiosyncrasies of the individual mind--essentially sum up the aim and method of modern science. The scientific man has above all things to strive at self-elimination in his judgments, to provide an

> argument which is as true for each individual mind as for his own.[85]

The historical memoirs that were published along side of the scientific memoirs in the pages of Biometrika created a "present moment" for the early-twentieth-century reader in which only the biometricians were seen to be engaging in the type of true collaborative scientific investigation that led to the destruction of individual bias and the production of absolutely objective knowledge about human evolution.

The story of the evolutionary and historical development of biometry, fashioned in the historical writing of the biometricians, was an attempt to solve one aspect of the problem of the individual--the potential for individual bias in the production of scientific knowledge. But there was a second aspect in which the individual was a recalcitrant obstacle to scientifically engineered social reform--the total unpredictability of the individual's reaction to environmental change. Accordingly, the science of biometry was designed to remove the unpredictable individual, not just from scientific research, but from both sides of the equation of social knowledge. How the biometricians attempted to erase the individual from the objects of their science is examined in the next two chapters.

## Notes

1. Karl Pearson, "Walter Frank Raphael Weldon, 1860-1906," Biometrika 5 (1906): 1-52. pp. 2-3.

2. Ibid., p. 3.

3. Ibid., p. 1.

4. Ibid., p. 2.

5. Ibid.

6. See Figure 4 in Chapter 3.

7. Karl Pearson, "Weldon," p. 2.

8. Ibid.

9. Ibid., p. 3.

10. Ibid., p. 4.

11. See Chapter 3.

12. The inheritance of an ability to make contributions from "outside" a given area of scientific expertise was also developed in the representations of Francis Galton as biometry's grandfather. See Chapter 3.

13. Pearson, "Weldon," p. 4.

14. Ibid., p. 5.

15. Ibid.

16. Ibid., p. 3.

17. Ibid.

18. Ibid., p. 6.

19. Ibid., pp. 6-7.

20. Ibid., p. 6.

21. Ibid., p. 7.

22. Ibid., p. 8.

23. Pearson, "Weldon," p. 8.

24. Ibid.

25. Ibid., p. 9.

26. Ibid.

27. Ibid., p. 11.

28. Ibid.

29. Ibid.

30. Ibid., p. 12.

31. Ibid.

32. Ibid.

33. Ibid., pp. 13-14.

34. Ibid., p. 14.

36. The publications included: "The Coelem and Nephridia of _Palaemon serratus_," _Journal of Marine Biological Association_ I (1889): 162-168, a paper which hinted at having correlation as its subject; "The Renal Organs of certain Decapod Crustacea, _Quarterly Journal of Microscopic Science_ 32 (1891): 279-291, which proved to be Weldon's last strictly morphological paper; and "The Formation of the Germ Layers in _Crangon vulgaris_, _Quarterly Journal of Microscopic Science_ 33 (1892): 343-363, his only piece of work on invertebrate embryology.

36. Pearson, "Weldon," p. 14.

37. Ibid., p. 15.

38. Ibid.

39. Ibid.

40. Pearson's account of the closing of Weldon's Cambridge work was extended by a footnote, which described the "general influence of Weldon at Cambridge." The footnote offered another genealogy of sorts--a role call of Weldon's intellectual progeny--his "wild oats"--sown at Cambridge. The list includes E.A. Shipley (who is credited by Pearson for assisting with this memoir) and William Bateson, who would become, in Pearson's narrative, the prodigal son.

41. Pearson, "Weldon," p. 15.

42. Ibid., p. 16.

43. Ibid., p. 17. The two papers are "The Variations occurring in certain Decapod Crustacea, Crangon vulgaris". R.S. Proceedings XLVII (1890): 445-453, which Pearson described as the first to apply the methods of Galton to other zoological types than man, and "Certain correlated Variations in Cragnon vulgaris, R.S. Proceedings LI (1892): 2-21, which Pearson credited as containing the first coefficients of organic correlation, that is, the numerical measures of the degree of interrelation between two organs or characters in the same individual.

44. Pearson, "Weldon", p. 17.

45. Ibid.

46. Ibid., p. 18.

47. Ibid.

48. Ibid.

49. Ibid. The paper was Weldon's "On certain correlated Variations in Carcinus moenas," R.S. Proceedings LIV (1893): 318-329.

50. Ibid., p. 19.

51. For an explicit statement on the importance of the elimination of the individual in Pearson's philosophy of science, see Karl Pearson, The Grammar of Science, especially the section of the introduction sub-titled "Science and Citizenship," p 11.

52. Pearson, "Weldon," p. 19.

53. Ibid., pp. 19-20.

54. Ibid., p. 20.

55. Ibid., pp. 22-23.

56. Ibid., p. 23.

57. Ibid.

58. Ibid.

59. Ibid., p. 28.

60. Ibid.

61. Ibid., p. 27.

62. Ibid., p. 29.

63. Ibid.

64. Ibid., p. 30.

65. Ibid., pp. 30-31.

66. Ibid., p. 30.

67. Ibid., p. 31.

68. Ibid., p. 32.

69. Ibid.

70. Ibid., p. 34.

71. Ibid., p. 46.

72. Ibid.

73. Ibid.

74. Ibid.

75. Ibid., pp. 39-40.

76. Ibid.

77. Ibid., p. 45.

78. Ibid.

79. Ibid.

80. Ibid., p. 50.

81. Ibid.

82. This verse appeared at the end of the memoir, p. 50.

83. Karl Pearson, The Grammar of Science (London: Dent & Sons, 1937), pp. 118-119. This edition used the text of the second edition of 1900. The Grammar was first published in 1892.

84. Ibid., p. 70.

85. Ibid., p. 11.

# CHAPTER 5
# SHIFTING SUBJECTS AND CHANGING OBJECTS: BIOMETRY IN THE HISTORY OF STATISTICAL THOUGHT.

Chapters 1 and 2 examined the relationship between historical writing and the historical record, and concluded that the identities of both the researcher and the objects researched are fashioned in the process of historical writing. Chapters 3 and 4 investigated the ways in which the history of the human species, and the history and philosophy of science, provided both rhetorical resources and contexts for the identification of the science of biometry and of its practitioners. The next two chapters will extend the analysis into the practice of biometry, and inquire about the relationship between the scientific researcher, scientific knowledge, and the objects that bear the burden of representing that knowledge.

## Probability and the Science of Society

Thanks to the substantial volume of work done in the last decade concerning the history of statistical thinking, we can articulate a general narrative of the development of mathematical statistics, and of the biometricians' role in that development.[1] The

narrative suggests that the originators of statistical science were social reformers with practical goals. A tradition of "social numbers" dates back to the 1660s when it was known as "political arithmetic." While often employed in the calculation of insurance and annuity rates, its broader goal was to provide empirical knowledge to serve as the foundation of sound state policy.[2]

In the eighteenth century, political arithmetic became increasingly involved in the varied controversies surrounding the "population question," as numerical investigators attempted to create reliable methods for measuring population size and for determining its rate of increase or decline. This type of political arithmetic gave way, in the nineteenth century, to "statistics," a term which described both the mathematical techniques for analyzing data derived from probability theory, and the numerical investigation of society and its problems.[3]

The mathematical theory of probability was an old and respectable area of study long before the creation of a sophisticated science of mathematical statistics. Most of the early work in probability studies concerned data drawn from dice-tossing and other games of chance. Prior to its incorporation into efforts to create a science of society, the only practical application of

probability theory was in actuarial work, where the laws of probability were linked to vital data, such as mortality rates, and applied to calculate insurance rates.[4]

The developments that eventually linked the mathematical theory of probability with efforts to create a science of society involved a transformation of both pursuits. In the eighteenth century, the subject matter of both mathematical probability and the "moral sciences" was the judgments and decisions of the rational individual. The eighteenth-century probabilists, drawing on traditional conceptions from rules of evidence in jurisprudence, conceived of their calculus as a quantification of degrees of certainty. This approach was in harmony with that of Enlightenment "moral science" which, also taking the rational individual as its subject matter, attempted to bring individual behavior into harmony with "natural law" by teaching individuals to recognize, and act on the basis of, enlightened self-interest.[5]

Lorraine Daston has pointed out that this "classical" interpretation of probability as an index of reasonable belief rested upon two assumptions: "first, that associationist psychology guaranteed the direct proportion between objective experience and subjective belief; and second, that good sense was monolithic and a

constant for the fortunate few who enjoyed it."[6] The first assumption refers to the theory that "reasonable belief" in the correlation of two events was directly and proportionally associated with the frequency with which such a correlation was observed in everyday experience. The second refers to the belief that "good sense" was a self-evident commodity possessed by the intellectual elite of a society.

Neither assumption survived the tumultuous events at the close of the eighteenth century. Rather, objective experience and subjective belief began to be understood as diametric opposites, as mathematicians and philosophers began to articulate a "frequentist" interpretation of mathematical probability, that is, they interpreted probability as a measure of observed frequencies, and began to oppose their results to "subjective" impressions.[7]

Efforts to create a science of society underwent a similar transformation. The psychological framework of eighteenth-century moral science and its desire for a computation of self-interest was abandoned in favor of the sociological framework exemplified by Auguste Comte's science of society and its search for immutable social laws.[8] The significance of the shift is summed up by Daston:

> For the eighteenth-century thinker, society was law-governed _because_ it was an aggregate of

> rational individuals; for his nineteenth-century counterpart, society was law-governed in spite of its irrational individual members.[9]

Nineteenth-century versions of probability theory and social science were brought together in the work of the Belgian statistician Adolphe Quetelet. Quetelet's work was firmly within the tradition of practical social reform. It has been described as an attempt to create a "social physics...in the spirit of gradualist liberalism," by developing "an extravagant system of metaphors and similes linking social domain to the theories and even the mathematics of physics and astronomy."[10]

Quetelet pursued his goal to create a true science of society in the image of physics and astronomy by examining such seemingly random events as crime, marriage, and suicide, not as individual behavior, but as aggregate behavior. By doing so, Quetelet was able to treat individual decisions (to commit a crime, marry, or commit suicide) in the same way that astronomers treated individual observational errors. Quetelet reunited probability theory and social science by reinterpreting the "probabilistic error function," the mathematical equation that describes the distribution curve later known as Gaussian, bell-shaped, or normal curve. The equation that describes the normal curve was introduced into probability theory by Abraham de Moivre

around 1733.[11] It was used as a method of reducing observational error in astronomical observations by both LeGendre and Gauss at the beginning of the nineteenth century.[12] Quetelet reinterpreted the probabilistic error function, treating it as a mathematical description of a law of genuine variation instead of observational error, and used it to demonstrate that seemingly random and unpredictable decisions were actually made with a regularity that could be mathematically understood and predicted.[13]

While Quetelet remained firmly in the probabilist and astronomical tradition by using the error law as a means of negating variation, other nineteenth-century explorers of aggregate phenomena soon adapted it to research programs that made genuine variation the focus. One such area was the development of kinetic theory, including Boltzmann's reduction of the second law of thermodynamics to mechanics and probability theory. Another was the quantitative study of heredity, the central interest of the biometric research program.[14]

## Beginnings of the Biometric Approach

Biometry was the offspring of a crossing of the search for a science of society with Darwinian evolutionism in the context of late-Victorian Britain. The early Victorian period had seen the advent of a "statistical movement" in Britain which, although

relatively short-lived, contributed much to the development of a British tradition of empirical social science research.[15] This movement was not characterized by sophisticated mathematical methods. The term statistics had not yet acquired such a connotation. The 1797 edition of the Encyclopedia Britannica, for example, defined statistics as a "word lately introduced to express a view or survey of any kingdom, country or parish."[16]

This form of statistics flourished in the 1830s, the decade of the great debate over the "condition of England" and the years that produced the Reform Act of 1832, the Factory Act of 1833, and the new Poor Law of 1834. A statistical office was set up under the Board of Trade in 1832, and a General Register Office was created in 1837 to collect vital statistics and to supervise a greatly expanded census that began in 1841. A statistical section of the British Association for the Advancement of Science was founded in 1833. In 1834, the Statistical Society of London (later the Royal Statistical Society) was formed through the efforts of Thomas Malthus, Charles Babbage, Richard Jones, W.H. Sykes, and J.E. Drinkwater.[17]

The numerical investigators of the Victorian period, called "statists" until very late in the century, were of an even more practical bent than their

predecessors. They tended to be business and professional men, reformers and bureaucrats, nineteenth-century liberals dedicated to uncovering the hidden order in apparently chaotic social phenomena. Cullen has argued that the early Victorian statistical movement should be seen as a group of social reformers producing and utilizing facts to advance their programs:

> The statisticians wanted to contribute more than voluntary and legislative action in the fields of public health and education: they were also free traders, supporters of the new poor law,...opposed to trade unions and working class radicals, suspicious of factory acts.[18]

The developing science of statistics, with its claims of mathematical rigor and of inherent objectivity, seemed to provide the means for such a task.

In order to separate their new science from the old "political economy," statists emphasized the objectivity of statistics. The motto of the Statistical Society of London was Aliis exterendum--to be thrashed out by others--and its council declared that the first and most essential rule of procedure was to exclude all opinions and to accept only facts.[19] Emphasis on the observation and the quantification of statistical regularities in society hardened into the concept of "statistical law" in the British context in the 1850s, when laissez-faire liberalism was at its height.

At the same time, the notion of social science began to flourish in Britain in the form of "conjectural

history." Works like Henry Thomas Buckle's History of Civilization in Europe (1857-61) regarded the historical development of society to have been largely beyond the influence of individual political actors and actions.[20] By the second half of the nineteenth century, the concept of statistical law was so much a part of the British social and scientific thought that researchers in the physical and biological sciences solved specific research problems by drawing on the social science of statistics.[21]

In the middle of the nineteenth century, however, statistical theory as a scientific specialty was still effectively non-existent in Britain. The link with developments on the Continent came through the work of William Farr, whose main interests were in hygiene and medical statistics, and in the notoriously diverse interests of biometry's grandfather figure, Francis Galton, where the social concerns and numerical proclivities of the statists were combined with eugenic concerns for the future of the human race.[22]

So convinced was Galton that only the objective rigor of a mathematical science of statistics would lead to any useful results, that he proposed, in 1877, that section F (statistics and economic science) be expelled from the British Association for the Advancement of Science because he was convinced that the members of the

statistical societies had no inclination to master the necessary mathematics.[23] For his own part, Galton spearheaded the biometric use of the statistical methods of error analysis, using them as a means of analyzing "real" variation in the context of eugenic concerns which were informed by the theory of evolution by natural selection. For example, Galton first used the error law in his work on the inheritance of "genius" (the results of which were published in 1869) in which he sought to demonstrate that both mental and physical characteristics were inherited, and distributed throughout the population in the manner described by the normal curve.[24]

Galton quickly became frustrated with the measures of error theory. They were designed to eliminate variation, the very focus of Galton's interest. Additionally, many of the attributes that interested Galton (like the moral attributes of human beings) could not be measured directly, but only rank ordered. To solve the problem, Galton developed his method of statistical analysis by "intercomparison." In this method, qualitative comparison produced a rank-order of the individuals or objects in a sample according to some quality they all possessed in some degree. From the rank-order, Galton reasoned that

> The object then found to occupy the middle position of the series must possess the quality in

> such a degree that the number of objects in the series that have more of it is equal to that of those that have less of it. In other words, it represents the mean value of the series in at least one of the many senses in which that term may be used.[25]

Galton later adopted Cournot's term "median" for this middle term (the expected mean and median are, in fact, identical as long as the normal law prevails), and went on to use what would later be termed the "quartiles"--the individuals or objects one-fourth and three-fourths of the way along the curve--to represent the probable error of the series.[26] These innovations greatly simplified the statistical study of human populations. Porter sums up their impact:

> Using the method of intercomparison, it no longer was necessary to measure directly every individual in a group, and record all pieces of information separately. The presumptive applicability of the error law implied that only two pieces of information need be known in order to characterize the entire distribution....These two values, mean and quartile, were sufficient to characterize or compare the populations.[27]

In developing his method of intercomparison, Galton had adapted the error law to deal with rank-ordered qualitative variation and, following Quetelet, refashioned probability theory as a means of studying variation rather than eliminating it. His subsequent invention of the concepts of correlation and regression extended the reach and vision of probability theory. Traditional error theorists worked predominantly with distributions of one variable or, at most, of mutually

independent variables. Regression and correlation, opened the door to the statistical analysis of two or more dependent variables.

In statistical theory, the theory of correlation is a theory of the association or relatedness of two (or more) series of numbers. For example, we know that height and weight are to some degree related. Mathematical analysis of typical patterns formed by series of measurements of heights and weights yields information about the degree of relatedness, or degree of correlation. The "correlation co-efficient" expresses the degree of correlation between the two variables in a given series.[28]

The variables that concerned Galton were those of human heredity. His understanding of this process rested on his cousin Charles Darwin's theory of pangenesis, a theory of heredity in which discrete particles or "gemmules" corresponded to individual inheritable characteristics. Although the notion of gemmules made it a "particulate" theory of heredity, it was in its effects a "blending" theory, in that the offspring produced was essentially a blend of the characteristics of paternal and maternal contributions.[29] Given his empirical bent, Galton naturally decided to put Darwin's theory to an experimental test. Understanding Darwin to have argued

that the gemmules circulated in the bloodstream and were only collected in the genitals during sex, Galton conducted a series of blood transfusions between different varieties of rabbits. When these transfusions produced no effect on the rabbits or their offspring, Galton concluded that Darwin was mistaken. Continuing to understand the hereditary process as consisting essentially of the blending together of discrete hereditary gemmules drawn together through a complex process of selection involving affinities and attractions, Galton put forward his own account of pangenesis, locating the gemmules permanently in the sex organs.[30]

The origin and location of the gemmules was not of critical concern to Galton. His concern was the process by which the gemmules were selected in reproduction. Galton's understanding of this process was based on an elaborate series of analogies from the social realm. Eventually, Galton came to think of the process by which the gemmules were drawn together and selected for inclusion to be analogous to social processes of selection like military conscription. As Porter has explained, this conception allowed Galton to subject the whole process to statistical analysis:

> The supposition that heredity concerned the action of numerous gemmules, subject to processes that may be likened to drawings from an urn or conscription from the young men of a nation,

> placed his model in conformity with the classic derivation of the normal law of errors. The theory of mutual affinities provided structure to his notion of biological correlation. Most important, the theory of pangenesis provided the foundation for a model of inheritance based on the fundamental parameters of statistics--a mean, deriving from the average of parental gemmules or traits, and a certain irregular deviation.[31]

In short, what Galton was after was a predictive, quantitative theory of human heredity; what he needed was an accurate statistical analysis of the hereditary process.

In an effort to study heredity with simple and reliable data, Galton temporarily abandoned his preferred anthropometric data and turned to breeding experiments using peas. The experiments, commenced in early 1876, consisted of the cultivation of peas from previously measured seeds. From the measurements, Galton calculated the mean and probable error of the distribution of weight, and then made up "sets" of seeds of equal weight, ranging from "very small" to "giant." Galton mailed nine sets of the categorized seeds off to friends with instructions to plant them in uniform soil and to keep the "offspring" of each group separate. Galton received seven sets of "progeny" (two of the sets failed) back from his correspondents, and analyzed them in the same manner as he had analyzed the "parent" seeds.[32]

It was in the articulation of the results of this experiment that Galton made his first statement of what would come to be known as the "law of regression." Specifically, Galton observed that the offspring of each category of parent had weights distributed according to the error law, and that the dispersion or probable error of each group of offspring was the same, that is, it was independent of the size classification of the parents. However, the mean weight of each class of progeny was less extreme than that of their parents; the mean displacement of the offspring was always less than that of the parents. From this result, Galton concluded that the offspring had, on the average, "reverted" part way back to the mean for the entire race.[33]

When trying to decide what the implications of this reversion were for the hereditary process, Galton convinced himself that it must be the answer to a question that had been bothering him, and others: "How is it that although each individual does not as a rule leave his like behind him, yet successive generations resemble each other with great exactitude in all their general features?"[34] In other words, Galton wondered how a population was able to retain a stable character from generation to generation (he was aware that the curve representing the distribution of a particular character in a species ordinarily remains virtually

identical from one generation to the next), when the individuals of that population routinely produce offspring that vary from their parents. Galton believed that he had found the answer in the phenomenon of reversion. The variability of a generation of progeny that would tend to expand the distribution curve was balanced over time by a compression of the curve that would result from the tendency to revert.[35] In the process of solving for himself the problem of hereditary equilibrium, Galton developed the basic mathematical apparatus for analyzing variation that he would later recognize as a general method of correlation. The solution to the problem had come by conceptualizing the reproductive process as consisting of stages, and imagining the selection of the gemmules to be governed at each step by the law of error.[36]

After spending several years working on various psychological investigations, Galton returned to the statistical study of heredity in 1884. He wished to show that his law of reversion, or "regression" as he now called it, held for human populations.[37] In 1884, Galton set up an anthropometric laboratory at the International Health Exhibition held in South Kensington. Galton subjected some of the data he collected there, data on the heredity of stature, to the same kind of analysis he had used on the peas. The case

of human heredity was slightly more complicated since humans had two parental influences. Galton solved the problem by creating the "mid-parent"--a fictitious entity fashioned by taking the mean of the paternal and maternal heights, adjusting the female heights to allow for the greater mean and probable error of male height.[38]

The results of Galton's analysis confirmed that the law of regression held for human populations. There was, however, one peculiar and problematic aspect to the results: the regression from mid-parental height to offspring height was precisely twice as great as the regression from offspring to mid-parental height. Galton liked to recall his solution to the problem in a story in which a flash of insight came upon him at the same time as a sudden squall forced him to seek refuge in a cave. The insight essentially consisted of the realization "that the laws of heredity were solely concerned with deviations expressed in statistical units."[39] In short, Galton realized that the distribution of his fictitious mid-parent was not identical to that of individuals. The differing distributions were due to the fact that the mid-parent consisted of the mean height of the father and of the (adjusted) mother. Galton's story aside, MacKenzie has shown that Galton arrived at the mathematical solution

to the problem by plotting it out graphically and enlisting the help of the mathematician Hamilton Dickson to construct an equation that described the patterns. The equation that Dickson produced was that which today is written in terms of "r" (the correlation coefficient of "x" and "y"), and is know as the bivariate normal distribution.[40]

The mathematics of regression and correlation are identical, but it was more than a decade before Galton realized it. This is not so surprising since Galton's primary concern was not the creation of statistical techniques, but rather discovery of laws of heredity upon which a eugenics program could be based. The concept of correlation prevalent in biological discourse was a qualitative one based on observation. It had been observed by breeders that certain inheritable characteristics tended to be passed on together. The long legs and long neck of the race-horse, for example, was thought to be a product of the phenomenon of correlation. The famous and influential French paleontologist Georges Cuvier used the concept of correlation in his skeletal reconstructions done from incomplete fossil remains, and Charles Darwin used it to explain how certain traits that were not in themselves advantageous might survive natural selection, if they were correlated with useful traits.[41] Galton had used

such a basic Darwinian notion of correlation in his earliest works on heredity, such as Hereditary Genius, in that he assumed that the inheritance of certain traits was correlated.

The mathematical conception of correlation developed out of Galton's interest in personal identification. Galton's interest in the problem was renewed by the proposal of a system of identification proposed by the French anthropometrician and criminologist Alphonse Bertillon. The system that Bertillon proposed consisted of a plan to classify all criminals by measurements of selected body parts. Bertillon believed that his system of measurements would lead to a classification system that would divide the criminal population into 81 classes.[42]

Galton was immediately skeptical of Bertillon's plan; specifically he was concerned that Bertillon's measurements were not independent. For example, Bertillon's system relied on such measurements as height and length of the extremities. Galton's concern was generated by the simple observation that such traits (for example, long fingers, long legs and long feet) tended to appear together--that they were in some sense interdependent or correlated. This interdependency would hamper the effectiveness and usefulness of Bertillon's system.

Using both Bertillon's data and the massive amount of data collected over the years at Galton's own anthropometric laboratory, Galton attempted to measure the degree of interdependence or correlation of these inherited traits. Galton's solution, which he achieved by drawing a graph of stature against left cubit, led him to realize that he had seen such patterns before in his work on family resemblances. Further work led him to the realization that the problem of correlation was mathematically identical to the problem of regression.[43] The correlated variables regressed (height regressed on cubit, and cubit on height) in the same manner that offspring height regressed on mid-parental height. All that remained to be done to create a true measure of correlation, was to assure the reciprocity of the correlation (the correlation of height and cubit should be the same as the correlation of cubit and height). Galton accomplished this by adjusting for the different probable errors of the two variables involved, just as he had for regression. Once adjusted, either variable regressed linearly on the other and the coefficients of regression were equal. Improvements and advancements would be made by Galton's successors, but the essential concepts and methods of statistical correlation were now in place.[44]

## The New Science of Biometry

Determined to bring about nothing less than a "Reformation" in the full range of the social and biological sciences by insisting on greater mathematical rigor and scientific objectivity, Karl Pearson and W.F.R. Weldon created the science of biometry, and Biometrika: A Journal for the Statistical Study of Biological Problems. In doing so, they tapped into a general movement to remake those sciences in the image of the physical sciences through quantification. In general, the quantification movement was the result of a conjunction of two separate but related developments. The first was a widespread feeling among a younger generation of researchers in Britain who had been trained in the tradition of evolutionary morphology (centered around F.M. Balfour at Cambridge) that the morphological approach had reached a dead end. The second was the continuing development of a tradition of eugenics research begun by Galton. Weldon was the product of the former, and Pearson of the latter; in their collaboration, they combined the two.

Pearson and Weldon sought to develop an evolutionary science that was philosophically impeccable, according to Pearson's philosophy of science as articulated in his book The Grammar of Science, first published in 1892. The essential element of Pearson's

philosophy of science was a Machian positivism which denied the existence of any "thing in itself" underlying human perceptions of phenomena, and which argued that sequences of perceptions provide the whole basis of our knowledge. Accordingly, he argued that the criterion for proper scientific principles was their contribution to economy of thought, and that a statistical understanding of nature (exemplified by correlation--and not the chimera of causation) was the proper basis for all scientific knowledge.[45]

Porter has pointed out that there is also a sense in which Pearson followed in the tradition begun by Quetelet.

> Both agreed on the universality of number and on the absence of discontinuity. Both maintained that the task of science was not to chart a bold new course, but to study the laws of social development so that scientific policy might affirm them and remove all obstacles to their attainment. Although Pearson's hereditarianism contrasted with Quetelet's social ameliorism, both stressed that statistics was fundamentally a practical science.[46]

More specifically, however, all of Pearson's statistical work was done within the context of his perspective as a Darwinian evolutionist and, as MacKenzie stresses, a eugenicist.

Pearson's early statistical work did much to define the new mathematical field of statistics. His first statistical paper concerned what he considered the central problem of statistics--the form of natural

distributions. He became convinced that their form was described by the normal curve. "When a series of measurements gives rise to a normal curve," Pearson wrote, "we may probably assume something approaching a stable condition; there is production and destruction impartially around the mean."[47] Cases in which the distribution did not appear to be "normal" were interpreted as being heterogeneous (that is, made up of a collection of different species or varieties). Accordingly, Pearson set out to establish a technique for separating these non-normal distributions into what he presumed were their separate normal components. Weldon's measurements of Naples crabs, the distribution of whose foreheads seemed to indicate a nascent division into two types, was the inspiration for Pearson's work in this area.[48] In this context, Pearson can be understood to have been seeking a statistical criterion of speciation. As Porter points out, however, Pearson

> also entertained the possibility...that mathematical analysis would reveal a difference rather than a sum of normal curves as the best decomposition. In that case, the positive component would presumably represent the birth curve, and the negative a selective death curve.[49]

By the time Pearson wrote his second statistical paper, he had become impressed by the prevalence of asymmetrical or skew distributions. Pearson concluded that these skew distributions were actually more common

in nature than symmetrical curves and that they should not all be presumed to be signifying heterogeneity.[50] Pearson illustrated his understanding of skew curves by likening them to drawings from an urn without replacement--a metaphor that fellow statisticians such as Edgeworth and Lexis found "physically uninterpretable."[51] Pearson's work on skew distributions has had less impact on the development of mathematical statistics than a specific mathematical technique he developed to demonstrate their superiority. The technique is today known as the chi-square distribution, and it forms the basis of a test of the "goodness of fit" between a set of measures and a mathematical curve to which they are supposed to conform, subject to random errors.[52]

Pearson's most powerful and lasting contribution to mathematical statistics is undoubtedly his method of calculating the correlation coefficient--the now standard product-moment expression for the coefficient of correlation--derived in 1896.[53] In this work, Pearson's synthesis of statistics, evolutionary theory, and a science of society is clearly seen. The central subject of the paper is the role of reproductive selection in human evolution. In Pearson's view, human populations in modern civilization had been essentially removed from the direct effects of natural selection.

The directionality of human evolution was, therefore, determined not by natural selection, but by the complex correlations of human characteristics and attributes and the correlations of those with an inherited capacity for reproduction. What was necessary then, in Pearson's view, for a truly scientific science of society was an understanding (based on a foundation of unimpeachable empirical mathematics) of the correlations of organs, with all their complex interactions, and a quantitative understanding of the patterns in which characteristics were inherited. The science of biometry was created to provide that foundation. The major obstacle to biometry's success was an audience of biologists and social scientists who remained skeptical that a system of high-powered and hard to grasp statistical measurements (which had traditionally measured only the uncertainty of the observer) actually corresponded to biological objects in the natural world.

## Statistics and the Production of Natural Objects

The changes in subject matter that characterize the history of the development of mathematical statistics in the social and human sciences went hand-in-hand with subtle changes in the conception of the objects being measured and studied. That correlation raises a difficult question. How are we to describe the relationship between the mathematization of the

discourse of the human and social sciences and the changes in the objects which bore the knowledge produced by that discourse?

The question of the ontological status of objects in the world is one of the most basic questions in all philosophy. In Representing and Intervening, one of the more influential works in the recent philosophy of science, Ian Hacking suggested a strategy for dealing with this problem that has had considerable appeal to historians of science. Specifically, Hacking suggested that we might understand the relationship between scientific knowledge and the object, particularly in the case of the experimental sciences, by positing a distinction between the practice of science (a phase in which the researcher "intervenes" in the behavior of a natural object) and a phase where the scientist represents that object (in an after-the-act sort of way) in order to articulate the results of research.[54] This distinction is, however, difficult to maintain in the course of historical research, especially in the case of biometry. In the practice of biometry (and in the practice of the human an social sciences in general) the relationship between the researcher and the object of research was very different from the laboratory dynamic that served as Hacking's model. Further, close historical examination reveals that the various modes of

representation involved in the creation of biometry not only preceded any consensus about the identity of the objects, but (as we shall see) actually participated in the production of those objects.

Just how problematic the fixing of an object of study was for the late-nineteenth-century architects of the social and human sciences was understood and eloquently articulated by their contemporary, John Theodore Merz. In the opening pages of a section of his four volume History of European Thought in the Nineteenth Century entitled "The Statistical View of Nature," Merz contrasted the "scientific explorer" and the "practical man."

> The successful scientific explorer has always been the man who could single out some special thing for minute and detailed investigation, who could retire with one definite object...; he can say to the object of his choice, "Ah, linger still, thou art so fair"....The practical man cannot do this; he is always and everywhere met by the crowd of facts, by the relentlessly hurrying stream of events. What he requires is a grasp of numbers....Thus has arisen the science of large numbers or statistics[55]

At critical points in the historical development of statistical thinking, a number of definite objects which could be made to linger under the gaze of the practical scientific researcher were produced. In each case, the new object reflected a fundamental ambivalence over the relationship and status of the individual and the aggregate.

Throughout the seventeenth and most of the eighteenth centuries, political arithmetic was a tool in the service of the centralizing bureaucracies of monarchies. The object that it examined and analyzed was not the autonomous individual but the aggregate body of estates, thereby giving an empirical reality to the cultural distinctions upon which a maze of privileges and responsibilities rested.[56] The mix of classical probability and the moral sciences, on the other hand, examined "society" as the sum total of the autonomous decisions of individual reasoning processes.
As both probability and the science of society changed in the wake of the chaotic events of the end of the eighteenth century, so too did their conceptions of the proper objects of study. Both moved away from the decision making process of individuals to aggregate behaviors. Probabilists concentrated on observed frequencies of events, while society as an object of statistical study came to be understood as a body of practices and behaviors produced by recalcitrant custom and natural law. Statistical research in this era, including Malthus's influential work on the "population principle," reflected the new conception.[57]

This new conception of society both produced and justified the claim that only the statistical method would yield the reliable knowledge necessary to properly

rationalize the legal and civil codes governing that society. Given the new conception of society, no reform could hope to succeed without a historically grounded understanding of custom, and a scientifically based grasp of the relevant natural law.

The new conception of society was, however, a mixed blessing for the orderly aspirations of the statists. It assured the statist of the possibility of social progress and a sense of duty to apply his talents towards that end, but it also filled him with a sense of foreboding, that the task would not be accomplished easily, and that failure could mean that their society would fall into chaos. This tension, between an unflinching belief in the progress of scientific knowledge (and of the western civilization shaped by it) and an almost paranoid fear that all was about to crumble into chaos, is inherent in the new conception of the human and social sciences. It is also reflected in the changing conception of the problem of the individual in reformist ideology. The confidence of laissez-faire liberalism gave way to a more pessimistic view in the late nineteenth century. Rather than free the individual from the hindrances of state control, the goal of the new statistical reformers (including both liberals and Fabian socialists) was the rationalization of state intervention on the basis of objective and

empirical data. To accomplish this goal, the epiphenomenon of individual decisions had to be removed from the calculus of society. By the late-nineteenth century, the individual was seen as an altogether too capricious object to represent accurately the constancy of natural law.

But if the individual was to be removed as the object of a mathematized social and biological science, something had to stand in its place--the statistics had to describe something. Several solutions were fashioned. Quetelet solved the problem by producing l'homme moyen or "the average man." Quetelet defined this abstract being as "the average of all human attributes in a given country," and maintained that it "could be treated as the 'type' of the nation, the representative of a society in social sciences comparable to the center of gravity in physics."[58] In Quetelet's scheme, the average man could be assigned traits--a penchant for crime, for example--in a way that the individual could not. All that was needed was a way to locate this average man and a way to fix him as an object of study. Quetelet found the necessary mode of representation in the normal curve produced by the probabilistic error function.[59]

When the creators of the science of biometry took the lead in the development of statistical theory and

representation, they took the process of eliminating the individual further by introducing other new objects into the social and human sciences--objects of the Darwinian evolutionist, such as the "population" and "true breeding pedigrees," which were mathematized and reified.[60]

In the work of the biometricians, the normal curve (though produced by increasingly sophisticated mathematical formulas) continued to represent the elusive object of social and human science. However, instead of representing Quetelet's social type, the biometricians presented the normal curve (and the mathematics that created it) as a uniquely objective technology which allowed the character of a natural population to identify itself.

As Porter points out, Galton was a transitional figure in the use of the error curve.

> Galton often used the error curve precisely in the fashion developed by Quetelet, as a definition of type. He sought, for example, to ascertain whether the basic forms of fingerprints represented genuine differences of type by determining if the variation within each were governed by Quetelet's law.[61]

For Galton, the normal curve provided empirical evidence of the existence of stable true breeding types within the general population whose frequency could be measured through statistics and managed through eugenics. Galton thought of these types in a way similar to Quetelet's

conception of the average man. The bulge of the bell-shaped distribution represented the "normal" character of the population. The degree of deviance was represented by relative distance from the mean value.

For Quetelet, the normal curve described the average man--a statistical composite of the physical, moral, and intellectual traits of the entire society. Like the reasonable man of the eighteenth-century investigators, the average man served as a social standard that defined normality and allowed it to be quantified.[62] For Galton, the normal curve described a stable archetype which natural selection could not overcome. In the statistical population thinking of the biometry of Pearson and Weldon, however, the meaning of normality was significantly transformed. In its new context, normality was neither a state to which individuals aspired nor a description of a stable archetype. Rather it was merely a description of the relative frequency of a character within the population. The most frequently appearing character traits constituted the normal condition of the population.

The difference between Galton and Pearson's interpretations of the normal curve stemmed from their disagreement over the implications of regression for evolution by natural selection. Galton interpreted regression as proof that natural selection could produce

no lasting effect, and that evolution required some fundamental change in organic stability, such as a large mutation or "sport." Pearson and Weldon, on the other hand, had been drawn to Galton's methods in the first place as a means to the empirical confirmation of the operation of natural selection in natural populations. Accordingly, they contended that the observed statistical regression was not, as Galton had argued, towards some stable archetype, but rather towards the statistical average of the existing population at any given time, an average that was itself constantly changing.

In the biometric interpretation, therefore, neither normality nor deviance was necessarily desireable nor undesirable. The creators of biometry, with their extraordinary mathematical aptitude, middle-class English industriousness, and scientific temperament considered themselves to be as much "deviants" as the physically deformed and mentally handicapped. What was now important was the <u>character</u> of the deviance. Deviance deemed desireable for the health of the English race and nation was to be perpetuated by "positive" eugenic measures. Deviance deemed undesirable was to be eliminated by "negative" measures that would discourage or prevent the reproduction of individuals who perpetuated it. This transformation of the conception

of what the normal curve represented had a tremendous effect on the psychology of the social and human sciences. The "normal type" represented by the normal curve as conceived by Quetelet and Galton had a comforting resiliency. In their interpretation, the existence of deviant types was unfortunate, but the ideal type that embodied the character of the nation would always exist. In the interpretation of the biometricians, the fitness of the national stock became a desire which could never be completely fulfilled, while the ruination of the national character through the promulgation of undesirable deviance became a real and dire threat.

The new technology of biometric statistics was seen, in this context, to offer an objective method of discovering and representing the fundamental reality of the population at any given time. Appearing to remove the impressionistic and biased judgments of the individual researcher while simultaneously erasing the unmanageable and irrelevant uniqueness of individuals from the object of study, biometric statistics promised to be the only honest scorekeeper and reliable guide in the constant battle over the character of the race and the nation. In contrast, the work of those who lacked statistical expertise was seen to be both impressionistic and imprecise--qualities that were

intolerable in a game where the stakes were so high. In the future, researchers not fluent in the language and methods of statistics would either have to submit to the guidance of the new experts, or withdraw from any role in social planning.

One major obstacle stood between biometry and the authority necessary for its self-appointed task--a skeptical audience, the sympathetic as well as the hostile, had to be convinced of the reality of the objects represented by statistics. In an effort to substantiate that reality, the biometricians turned to another mode of representation--one which was quickly gaining the kind of authority that statistics desired, photography.

## Notes

1. The history of statistics as a sub-field in the history of science came into its own in the 1980s, largely as a result of what Donald MacKenzie has called "a remarkable scholarly experiment" (See Donald MacKenzie, "Probability and Statistics in Historical Perspective," Isis 80 (1989): 116-124. p. 117). The experiment was a year-long seminar, spanning the academic year 1982/83, which brought researchers from all over the world together at the Zentrum fur Interdisziplinare Forschung at the University of Bielefeld. The seminar provided the impetus for an out-pouring of publications that have given us a roughly consensual understanding of "the Probabilistic Revolution." The Bielefeld seminar led directly to the publication of: Lorenz Kruger, Lorraine J. Daston, and Michael Heidelberger, eds., The Probabilistic Revolution, Volume I: Ideas in History (Cambridge, MA/London: MIT Press, 1987); and Lorenz Kruger, Gerd Gigerenzer, and Mary S. Morgan, eds., The Probabilistic Revolution , Volume II: Ideas in Science (Cambridge,

MA/London: MIT Press, 1987). Several individual participants also published important works separately: Stephen Stigler, The History of Statistics: The Measurement of Uncertainty before 1900 (Cambridge, MA: Harvard University Press, 1986); Theodore M. Porter, The Rise of Statistical Thinking, 1820-1900 (Princeton, N.J.: Princeton University Press, 1986); and Lorraine J. Daston, Classical Probability in the Enlightenment (Princeton, N.J.: Princeton University Press, 1988).

2. Porter, Rise of Statistical Thinking, pp. 17-20. Examples of this kind of work include John Graunt's "Observation upon the Bills of Morality" (1676; 1st ed., 1662), reprinted in C.H. Hull, ed., The Economic Writings of Sir William Petty, 2 vols., Cambridge, Eng., 1899), vol. 2, pp. 395-396 (It is reprinted there because William Petty was believed to have had a hand in its writing), and in the works of Petty who coined the phrase "political arithmetic. (see Economic Writings). For more on political arithmetic, see Peter Buck, "Seventeenth-Century Political Arithmetic: Civil Strife and Vital Statistics," Isis 68 (1977): 67-84. For the role of probability theory during this period, see Lorraine J. Daston, "The Domestication of Risk: Mathematical Probability and Insurance 1650-1830," in Probabilistic Revolution, V.1, pp. 237-260.

3. Porter, Rise of Statistical Thinking, pp. 20-23. For an introduction to the population debate in England, see David V. Glass, Numbering the People: the 18th-Century Population Controversy and the Development of Census and Vital Statistics in Britain (London/New York: Gordon and Cremonesi, 1978).

4. Probability calculations based on mortality records were used to set rates for life insurance and annuity purchases ever since Edmond Halley published the first life table in 1693. See Porter, Rise of Statistical Thinking, pp. 71-88.

5. Lorraine J. Daston, "Rational Individuals versus Laws of Society: From Probability to Statistics," Probabilistic Revolution, vol. 1, pp. 295-304, especially pp. 295-298.

6. Ibid., p. 300.

7. Ibid., pp. 300-301.

8. Ibid.

9. Ibid., p. 295.

10. Porter, Rise of Statistical Thinking, p. 46.

11. Ibid., p. 93.

12. LeGendre announced the method in 1807. Gauss immediately responded that he had been using it since 1798. See Porter, Rise of Statistical Thinking, p. 95.

13. Porter, Rise of Statistical Thinking, pp. 47-55. Daston points out that Quetelet's work also required a reinterpretation of the relationship between probability theory and statistical data. "The empirical study of social regularities began as an antiprobabilistic approach....The stability of statistical ratios, expressing the divine order, were opposed to probabilities, representing the workings of mere chance....Not until the 1830s, were mathematicians and social theorists like Poisson and Quetelet able to dissociate stable statistical ratios from divine providence....Poisson severed statistical regularities from the argument from design by showing that such regularities were just what one would expect from the calculus of probabilities." Daston, "Rational Individuals," pp. 302-303.

14. Porter, Rise of Statistical Thinking, p. 110.

15. See M.J. Cullen, The Statistical Movement in Early Victorian Britain (Hassocks, Sussex: Harvest, 1975); and V. Hilts, "Statistics and Social Science," in Foundations of the Scientific Method: The Nineteenth Century, R.N, Giere and R.S. Westfall, eds., (Bloomington: University of Indiana Press, 1973), pp. 206-233.

16. Quoted in Cullen, Statistical Movement, pp. 10-11. The term Statistik was first used as a substantive by the Gottingen Professor Gottfried Achenwall in 1749. The anglicized form was introduced by John Sinclair, a member of a network of Presbyterian pastors whose collective labor led to the 21-volume Statistical Account of Scotland (Edinburgh, 1791-99). Porter, Rise of Statistical Thinking, pp. 23-24.

17. Porter, Rise of Statistical Thinking, p. 31.

18. Cullen, Statistical Movement. p. 147. See also Donald, MacKenzie, Statistics in Britain, 1865-1930 (Edinburgh: Edinburgh University Press, 1981), pp. 7-10; and Porter Rise of Statistical Thinking, pp. 18-39.

19. See Victor L. Hilts, "Alliis Exterendum, or, the Origins of the Statistical Society of London," Isis 69 (1978): 21-43.

20. See J.W. Burrows, Evolution and Society: A Study in Victorian Social Theory (London: Cambridge University Press, 1966), and J.D.Y. Peel, Herbert Spencer: The Evolution of a Sociologist (New York: Basic Books, 1971).

21. Porter, Rise of Statistical Thinking, pp. 65-70.

22. For Farr and his ties to Quetelet, see Bernard-Pierre Lecuyer, "Probability in Vital Statistics: Quetelet, Farr, and the Bertillons," in Probabilistic Revolution, v. 1. pp. 317-335.

23. Porter, Rise of Statistical Thinking, pp. 135-136.

24. Francis Galton, Hereditary Genius (London: Macmillan,1869). For Galton's contributions to statistics and to evolutionary biology, see Ruth Schwartz Cowan, "Francis Galton's Statistical Ideas: the Influences of Eugenics," Isis 63 (1972): 509-528; Ruth Schwartz Cowan, "Francis Galton's Contribution to Genetics," Journal of the History of Biology 5 (1972): 389-412; Ruth Schwartz Cowan, "Nature and Nurture: the Interplay of Biology and Politics in the Work of Francis Galton," Studies in the History of Biology 1 (1977): 133-208; P. Froggatt and N.C. Nevin, "Galton's Law of Ancestral Heredity: Its Influence on the Early Development of Human Genetics," History of Science 10 (1971): 1-27; MacKenzie, Statistics in Britain, p. 57; and Porter, Rise of Statistical Thinking, pp. 137-138. For Galton's life and work, there is D.W. Forrest, Francis Galton: the Life and Work of a Victorian Genius (London: Elek, 1974).

25. Francis Galton, "Statistics by Intercomparison, with Remarks on the Law of Frequency of Error," Philosophical Magazine, series 4, 49: 33-46. p. 34; as quoted in MacKenzie, Statistics in Britain, p. 58. For more on Galton's method of intercomparison, see also Porter, Rise of Statistical Thinking, pp. 140-145.

26. MacKenzie, Statistics in Britain, pp. 58-59; and Porter, Rise of Statistical Thinking, pp. 143-145. Galton used the architectural term `ogive' to designate the curve generated by this procedure.

27. Porter, Rise of Statistical Thinking, pp. 143-145.

28. "The coefficient of correlation ranges between +1 and -1. It is +1 if a high value for one variable implies a proportionately high value for the other (perfect correlation). It is -1 if a high value for one variable implies a proportionately low value for the other (perfect inverse correlation). It is zero if the two variables are unrelated (independent)." MacKenzie, Statistics in Britain, p. 246.

29. Darwin's theory of pangenesis is put forth in chapter 27 of Charles Darwin, Variation of Animals and Plants under Domestication (2 vols., London), 1868. See also, Gerald L. Geison, "Darwin and Heredity: The Evolution of His Hypothesis of Pangenesis," Journal of the History of Medicine and Allied Sciences 24 (1969): 375-411.

30. Francis Galton, "A Theory of Heredity," Contemporary Review 27 (1875): 80-95. Darwin responded by saying that he had never claimed that the gemmules had to be in the bloodstream. See William B. Provine, The Origins of Theoretical Population Genetics (Chicago: The University of Chicago Press), 1971. pp. 16-17.

31. Porter, Rise of Statistical Thinking, pp. 285-286.

32. MacKenzie, Statistics in Britain, pp. 60-61; and Porter, Rise of Statistical Thinking, pp. 286-288.

33. Galton reported his results of the pea experiments in a lecture delivered at the Royal Institution on 9 February 1877, which was published as Francis Galton, "Typical Laws of Heredity," Proceedings of the Royal Institution 8 (1877): 282-301. For description and analysis see MacKenzie, Statistics in Britain, pp. 60-61; and Porter, Rise of Statistical Thinking, pp. 286-287.

34. Francis Galton, "Typical Laws of Heredity," PRI 8 (1877): 282-301; reprinted in Nature 15 (1877): 492-495, 512-514, 532-533, p. 512.

35. For a mathematical demonstration of how this works, see MacKenzie, Statistics in Britain, p. 62; and Porter, Rise of Statistical Thinking, p. 288.

36. Porter, Rise of Statistical Thinking, p. 288.

37. The precise significance of the change in terminology is unclear. Porter has suggested that the change reflected a new conviction that "return to the mean reflected an inherent stability of type, and not

merely the reappearance of remote ancestral gemmules." Porter, Rise of Statistical Thinking, p. 289. Ruth Schwarz-Cowan has suggested that it indicates a realization on Galton's part of a greater generality of the relationship he had found. See Ruth Schwarz-Cowan, "Francis Galton's Statistical Ideas: The Influence of Eugenics," Isis 63 (1972): 509-528, p. 520.

38. For example, in order to calculate the height of a human mid-parent for a given sample, Galton multiplied the mother's height by 1.08 and took the mean of that and the father's height. See Francis Galton, "Address to the Anthropological Section of the British Association," Nature 32 (1885): 507-510, p. 507.

39. Francis Galton, Memories of My Life, 3rd ed., (London: Macmillan, 1909), p. 300.

40. This equation was reported in Francis Galton, "Family Likeness in Stature," Proceedings of the Royal Society 40 (1886):42-73, which includes an appendix by Hamilton Dickson, Ibid., 63-66. For an explanation of the mathematics from a modern perspective, see MacKenzie, Statistics in Britain (appendix 3), p. 231-234.

41. Peter J. Bowler, Evolution: The History of an Idea (Berkeley: The University of California Press, 1984), pp. 106-112 (for Cuvier), and p. 109 (for Darwin).

42. Porter, Rise of Statistical Thinking, p. 291.

43. Galton reported the results of this work in Francis Galton, "President's Address," Journal of the Anthropological Institute 18 (1889): 401-419; and Francis Galton, "Kinship and Correlation," North American Review 150 (1890): 419-431. For an analysis of Galton's mathematics from a modern perspective see MacKenzie, Statistics in Britain, p. 67; and Porter, Statistics in Britain, pp. 292-293.

44. MacKenzie, Statistics in Britain, p. 67.

45. For the development of Karl Pearson's philosophy of science see B.J. Norton, "Biology and Philosophy: the Methodological Foundations of Biometry," Journal of the History of Biology 8 (1975): 85-93; and B.J. Norton, "Metaphysics and Population Genetics: Karl Pearson and the Background to Fisher's Multi-factorial Theory of Inheritance," Annals of Science 32 (1975): 537-553.

46. Porter, Rise of Statistical Thinking, pp. 304-305.

47. Karl Pearson, "Contributions to the Mathematical Theory of Evolution," Philosophical Transactions A 185 (1884): 71-110; quoted in Porter, Rise of Statistical Thinking, p. 307.

48. The results of this work were eventually published in W.F.R. Weldon, "The Variations occurring in Certain Decapod Crustacea-I. Cragnon vulgaris," Proceedings of the Royal Society 47 (1890): 445-453; and W.F.R. Weldon, "Certain Correlated Variations in Cragnon vulgaris," Proceedings of the Royal Society 51 (1892): 2-21.

49. Porter, Rise of Statistical Thinking, pp. 307-308.

50. Karl Pearson, "Contributions to the Mathematical Theory of Evolution. II. Skew Variation and in Homogeneous Material," Philosophical Transactions A 186 (1895): 343-414.

51. Porter, Rise of Statistical Thinking, p. 308.

52. Porter, Rise of Statistical Thinking, p. 310. Pearson published this work in Karl Pearson, "On the Criterion that a given System of Deviations from the Probable in the Case of a Correlated System of Variables is such that it can be reasonably supposed to have arisen from Random Sampling," Philosophical Magazine, series 5, 50 (1900): 157-175. For closely related work. see Karl Pearson and L.N.G. Filon, "Mathematical Contributions to the Theory of Evolution. IV. On the Probable Errors of Frequency Constants and on the Influence of Random Selection on Variation and Correlation," Philosophical Transactions A 191 (1898): 229-311.

53. Karl Pearson, "Mathematical Contributions to the Theory of Evolution. III. Regression, Heredity, and Panmixia," Philosophical Transactions A 187 (1896): 253-318.

54. Ian Hacking, Representing and Intervening: Introductory Topics in the Philosophy of Natural Science (Cambridge/London: Cambridge University Press, 1983).

55. J.T. Merz, A History of European Thought in the Nineteenth Century, Vol. II (Edinburgh/London: Blackwood, 1903), pp. 554-555. Merz alluded here, without identifying it, to the ideal goal sought by Goethe's Faust.

56. Porter, Rise of Statistical Thinking, p. 25.

57. Thomas Robert Malthus, An Essay on the Principle of Population (2 vols., London). For more on the context of Malthus's work, see Stefan Collini, "Political Theory and the Science of Society in Victorian Britain," Historical Journal 23 (1980): 203-231. See also, Porter, Rise of Statistical Thinking, pp. 25-27.

58. Porter, Rise of Statistical Thinking, p. 52.

59. Porter, Rise of Statistical Thinking, p. 93. The equation that describes the normal curve was introduced into probability theory by Abraham de Moivre around 1733. It was used as a method of reducing observational error in astronomical observations by both LeGendre and Gauss at the beginning of the nineteenth century. LeGendre announced the method in 1807. Gauss immediately responded that he had been using it since 1798. Quetelet reinterpreted it as a means of describing a law of genuine variation.

60. Some historians of biology consider the development of what they call "population thinking" to have been a crucial step in the development of modern evolution theory. Its effect was, however, equally profound for the development of a statistically oriented social and human science. For an introduction into the literature of the creation of modern evolution theory, see Ernst Mayr, "Essay Review: The Recent Historiography of Genetics," Journal of the History of Biology 6 (1973): 125-155; and Ernst Mayr and William B. Provine (eds.), The Evolutionary Synthesis: Perspectives in the Unification of Biology (Cambridge, Mass.: Harvard University Press, 1980). See also V.B. Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," Journal of the History of Biology 25 (1992): 1-65.

61. Porter, Rise of Statistical Thinking, p. 139.

62. Daston, "Rational Individuals," p. 303.

# CHAPTER 6
## REPRESENTING THE REAL:
## STATISTICS AND PHOTOGRAPHY AS SCIENTIFIC EVIDENCE

The third volume of Biometrika (1904) contained 55 photographic plates, 50 of which depicted human skulls. The presence of these photographs is puzzling. They appeared in an article which continued a series of biometric explorations in the field of craniometry (about which I shall have more to say shortly), but it is not immediately apparent what they were meant to contribute. All of the important data--all of the information that a biometrician would need to analyze the population under investigation--appeared in the tables, charts, and graphs that surrounded the photographs.

Some of the strongest proponents of the so-called "new cultural history" have contended that to come face to face with such a puzzle--to confront a riddle or a joke that we do not get--is to confront the genuine otherness of a different place and time.[1] Following that line of argumentation, the presence of these photographs provides a key to the understanding of the historical milieu of the early biometricians. More specifically, they can be treated as surviving traces of

the rhetorical moves required to execute the biometrician's program for the complete reform of the human and social sciences. If we can understand the rhetorical function of these photographs, and of photography in general, in the biometric program, we will have grasped something significant about the place and time in which biometry was created, and about the nature of that transformation, the effects of which are still profoundly felt in western civilization.

## The Potential of Photography

The technical developments which led to the invention of photography required the solution to a variety of theoretical and instrumental problems in the fields of optics and chemistry. The engine that drove the process of innovation was an unprecedented demand, largely on the part of middle class citizens in Britain and France for images. When the process of the daguerreotype was made public by the French government in August 1839, entrepreneurs discovered the insatiability of that demand. The market for pictures expanded as fast as it could be supplied. Beard of London, who controlled the British patents on the daguerreotype, made a profit of 30,000 pounds in the second year of business.[2]

Theoretical limits to the supply of photographs were eliminated in the 1830s when William Henry Fox

Talbot, working independently from French innovators, developed the processes which led to the creation of paper print photographs. The collodion or wet-plate process, introduced in 1851, dramatically cut exposure and processing times, making it possible to mass-produce photographs and making the field more accessible to amateurs. The invention of faster dry-plates (like the collodion dry-plate in 1864, and the gelatin dry-plate in 1878) and of flexible film, decreased the difficulty and increased the availability of photographs. Fast-reacting, pre-manufactured plates freed photographers from the need of on-site darkrooms and tripods, as a flood of hand-held cameras introduced in the 1880s promised to make everyone a photographer.[3]

While the great profit potential of photography was being recognized and exploited by entrepreneurs, another kind of potential was being considered by the various institutions of an expanding European state--the potential for surveillance and control inherent in the ability of the photograph to function as identification and evidence. Today the photograph is a privileged form of representation in western cultures. It carries a degree of evidential authority rivaled only by statistics. Of course not just any photograph can function as evidence; there are rules that must be followed, criteria that must be met. The history of the

creation of those rules and criteria in the second half of the nineteenth century has only begun to attract the attention of historians. Early attempts to outline a narrative of that history stress the point that the articulation of evidential criteria for photography was bound up with the emergence of new institutions and new practices of observation and record-keeping. Together, these developments constituted a movement to standardize and professionalize a network of social institutions (including the police, prisons, asylums, hospitals, departments of public health, schools, and factories) and the relatively new web of social and human sciences (including sanitation, criminology, psychiatry, comparative anatomy, anthropology and anthropometry).[4] It is within that context that the rhetorical function of photography in the practice of biometry can be understood.

## Photography and Statistics in the Name of Order

The potential of photography to aid in the rationalization, reform, and policing of society was recognized early in its history. British police agencies employed civilian photographers as early as the 1840s.[5] The photographic documentation of prisoners became common in the 1860s, and its potential as a means of identifying "dangerous classes" of people was widely recognized as early as the 1840s.[6] The adoption of

photography by state institutions in the mid-nineteenth century took place in an intellectual climate characterized by the general belief that the surface of the body, especially the face and head, bore the outward signs of inner character. In such a milieu, sciences such as physiognomy and phrenology flourished. Physiognomy concentrated on the overall shape of the head, profile, and facial features (ears, chin, eyes, nose, etc.). It sought to fit individuals into categories of physical type which corresponded to intellectual and moral types. Phrenology was concerned with the topography of the skull and sought to find correspondences between that topography and what were thought to be brain centers correlated with specific mental functions. The general hermeneutic paradigm that connects the two is a belief that the surface of individual bodies could be analytically categorized into existing types.[7]

Photography was first incorporated into the interpretive analysis of physiognomy by Eliza Farnham. The matron of the women's prison at Sing Sing and devoted to the reform of the penal system, Farnham commissioned Matthew Brady to make a series of portraits of inmates at two New York prisons. Engravings from the photographs were published in the appendices produced by Farnham for a new edition of Marmaduke Sampson's

Rationale of Crime and its Appropriate Treatment, Being a Treatise on Criminal Jurisprudence Considered in Relation to Cerebral Organization, published in 1846. The photographs aided in the general purpose of the treatise, which was to identify types of criminals and categorize them in terms of those that could be rehabilitated and those that could not.[8]

The use of photography in the clinic began in earnest in Britain in the 1850s, when Dr. Hugh Welch Diamond, founding member of the Royal Photographic Society and resident superintendent of the Female Department of the Surrey County Lunatic Asylum, popularized the practice of photographing the inmates of asylums, to aid in identifying, categorizing, and recording individual cases.[9]

Photographic documentation on the part of social institutions expanded greatly in the 1870s. Local governmental agencies commissioned photographic surveys of working-class living conditions. Large prisons set up their own studios and employed staff photographers, and orphanages like John Barnardo's "Home for Destitute Lads" followed the example by keeping detailed photographic records of their inmates.[10] It was, however, in the context of criminology and policing that photography and statistics were brought most directly into association in the 1880s.

Alphonse Bertillon, director of the Identification Bureau of the Paris Prefecture of Police and son of the prominent anthropometrician Louis Adolphe Bertillon, utilized photographic portraiture, anthropometric measurements, and textual description to create individual record cards which, when organized by an elaborate filing and indexing system, constituted a comprehensive system of criminal identification. For Bertillon, whose imperatives were practical, the problem was how to retrieve the individual out of an immense number of photographs. The system he developed (alternately called "Bertillonage" and the "signaletic notice") began with a anthropometric description of the individual criminal consisting of eleven measurements of selected body parts recorded as a numerical sequence. The anthropometric data was supplemented by a standardized shorthand description noting any distinguishing marks, and by two photographic portraits showing the individual from both frontal and profile view. The measurements were then organized into successive sub-divided categories, each based on a qualitative evaluation of below-average, average, and above average measures. In this way, the cards could be used to access records of repeat offenders and to identify suspects from the descriptions of witnesses.

The photographs and descriptive text aided in "positive" identification.[11]

As noted in Chapter 5, Bertillon's system had been of great interest to Francis Galton. Galton's insight that the variables of Bertillon's system were dependent on one another led Galton to the recognition that the mathematics of regression, which he had formulated a decade earlier, were identical to the mathematics of correlation.[12] Galton also shared Bertillon's interest in photography as a means of identification and was among the first to mix statistics and photography in the context of British anthropometry and anthropology. Galton's approach, however, was significantly different from Bertillon's; specifically, Galton invented the composite photograph.[13] The composite photograph is an image produced by superimposing a series of individual portraits by photographing them one after another, using a single photographic plate. The length of exposure depends on the number of individual portraits. For example a composite photograph of a dozen portraits would be made by photographing each at one-twelfth of the correct time. The effect is a composite portrait in which characteristics that the individual images had in common will be most clearly visible, while characteristics peculiar to one individual will be under-exposed and, therefore, nearly invisible. Like

statistics, the clarity and reliability of the image increases with the number of individuals sampled. As Galton explained in his Inquiries into the Human Faculty and Its Development (first published in 1883 and brought out in a second edition in 1907), the effect was to "bring into evidence all the traits in which there is agreement and to leave but a ghost of a trace of individual peculiarities."[14]

The middle section of the page of composite photographs published in the second edition of Galton's Inquiries illustrates Galton's approach (see Figure 8). To provide evidence for the existence of healthy, diseased, and criminal "types" within the English population, Galton employed composite photography to allow the image of those types to make themselves visible. To produce a picture of health (literally), Galton combined the portraits of twelve officers and eleven privates, all from the corps of Royal Engineers. As individuals, the men "differed considerably in feature," but the composite produced an image "having an expression of considerable vigour, resolution, intelligence, and frankness"--an expression of the "bodily and mental qualifications required for admission into their select corps, and their generally British descent."[15] For contrast, Galton provided images of disease and criminality, which gave evidence for the

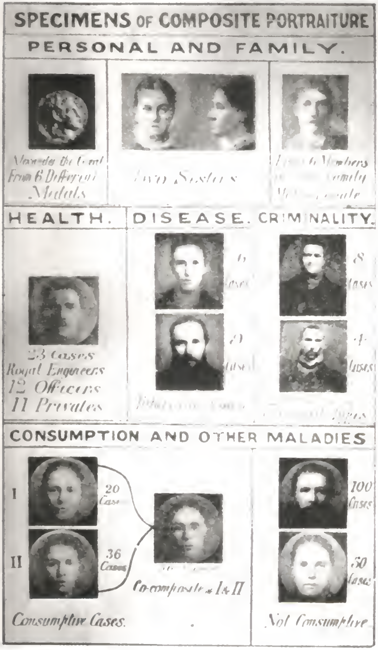



Figure 8. Francis Galton's Composite Photographs. Published in <u>Inquiries into the Human Faculty and Its Development</u>, 2nd edition, 1907.

existence within the English population of distinct types susceptible to disease and with a penchant for crime.

For Galton, the composite photograph functioned like the normal curve. Both provided empirical evidence of the existence of true breeding types within the general population whose frequency could be measured through statistics and managed through eugenics. Because (as we have seen in Chapter 5) Galton thought the normal type to be held stable by regression, he believed that the composite photograph of the healthy type provided

> a clue to the direction in which the stock of the English race might be most easily improved. It is the essential notion of a race that there should be some ideal typical form from which individuals may deviate in all directions, but about which they chiefly cluster, and towards which their descendants will continue to cluster. The easiest direction in which the race can be improved is towards that central type, because nothing new has to be sought out.[16]

Allan Sekula has argued that the differences between Bertillon's and Galton's approaches to photographic identification in criminology are indicative of "two different approaches to the photographic representation of the criminal body."[17] In that analysis, Bertillon's approach is identified with the practical imperatives of "the technicians of crime [who] sought knowledge and mastery of individual criminals."[18] Galton's approach is alternatively

identified with the theoretical aims of "would-be scientists of crime [who] sought a mastery and knowledge of and elusive criminal type."[19] Finally, Sekula asserts that "the projects of Bertillon and Galton constitute two methodological poles of the positivist attempts to define and regulate social deviance. Bertillon sought to individuate....Galton sought to visualize the generic evidence of hereditary laws."[20]

More than simply methodological poles, however, the differences and similarities of Bertillon's and Galton's projects indicate a fundamental tension inherent in the positivist program to remake the human and social sciences in the image of the physical sciences. The constantly reiterated goal was to ensure the practicality of the human and social sciences, and therefore to keep them relevant to social programs and policies that would effect every individual in society. The twin goals of unmediated observation and unrelenting quantification, however, demanded the erasure of the taint of the individual at every level.

As noted in Chapter 5, Karl Pearson and the biometricians rejected Galton's interpretation of the normal curve. They also rejected his use of composite photography. Both were rejected because they were understood to be representing the wrong kind of object. While Galton saw the normal curve as representing a

stable, "ideal typical form" present in populations, Pearson saw it as representing the dominant character of a population at any given time--a character that was subject to change under selective pressure. For Pearson then, the analogy Galton saw between statistics and composite portraiture was flawed. To Pearson's mind the proper analogy was not the portrait, an image of permanent familial essence, but the snapshot, a frozen glimpse of the harsh and ever-changing reality.

In some ways, these differences may be understood to be generational. Galton belonged to a generation that came to intellectual maturity in the mid-nineteenth century, and shared in the general belief underlying physiognomic and phrenologic studies that the surface of individual bodies could be analytically categorized into existing types. Galton's composite photography experiments were an effort to allow that type to become clearly visible by piling surface upon surface until similarity drowned out difference. By the close of the century, investigators of Pearson's generation had become convinced that neither the qualitative analytical skills of the individual researcher, nor the peculiarities of the individual object of research could relied upon. Accordingly, there was a shift away from the qualitative and "superficial" investigations of physiognomy and phrenology, and toward the quantitative

science of craniometry, which sought to find a correlation between mental capabilities and the volume capacity of the skull. Pearson and the biometricians made craniometry (or more specifically, the introduction of rigorous statistical methods to craniometry) one of its central concerns.

Similar changes were also occurring in the realm of representation. By the time that Pearson and Weldon began publishing Biometrika in 1901, the rules for producing a photograph with evidential authority were being codified. The guidelines for the production of photographs to be used as evidence in a court of law, for example, stressed such characteristics as sharpness and frontality. Evidential photographs were to be taken at eye level and photographers were to avoid harsh or exaggerated lighting. They were to use standard processing, and to avoid overt manipulation.[21] In sum, the evidential authority of a photograph--its ability to guarantee the existence of the object represented--hinged on its ability to create the illusion that it had been produced in the absence of an author; to appear to be a reflection of a previous moment, a moment captured forever by a camera which had been merely "triggered" by the photographer.[22]

In sum, the desire for objective, unmediated observation, and the consequent effort to erase the

individual, permeated the movement to professionalize the human and social sciences in the latter part of the nineteenth century. That desire provided the context for the rise of statistical and photographic modes of representation in the social and human sciences. Statistics and photography possessed evidential authority only when they appeared to offer an unmediated reflection of reality, uncluttered by individual peculiarities and unsoiled by individual interpretation. The composite photograph failed to fulfill that criteria. The fuzzy edges of the composite photograph testified to the process by which individual portraits were selected, registered, and superimposed, by a photographer's hand--by the author of the image. It was, therefore, not the manipulative image of the composite but rather the reflective image of police and clinical photograph that the biometricians desired.

## Authority by Association

By the time Pearson and Weldon founded Biometrika, photographs produced along strict guidelines developed within the courts were accepted as possessing unique evidential authority. To Pearson and Weldon, statistics provided a form of knowledge worthy of the same authoritative voice for the same reasons. Statistics, they believed, also eliminated both the bias of the individual researcher (through rigorous quantification)

and the morass of individual peculiarities from the object researched. Photography, however, is a mode of representation uniquely suited to the task of creating the illusion of objective, unmediated observation. The evidential photograph, produced to hide the hand of its maker, seems to provide the viewer with a direct visual experience of a prior event; it seems to recreate the experience of first-hand observation. Even a viewer looking upon his or her first photograph can experience its effect. In contrast, the viewer unfamiliar with statistical data and techniques is liable to experience only bafflement in an encounter with statistics. The use of photographs in biometric papers was a rhetorical solution to the problem. The photographs that were woven into the biometric texts functioned to persuade the reader that, like photography, statistical representation possessed an evidential authority based on its assumed ability to provide unmediated observation of natural reality. The photographs were a rhetorical strategy of authority by association.

The association of statistics with the authority of photography was not always easy. The objects of biometric statistics were not easily photographed. Sometimes the pristine visuality of the photograph had to be compromised with textual narrative. Such was the case with the photographs that accompanied a lengthy

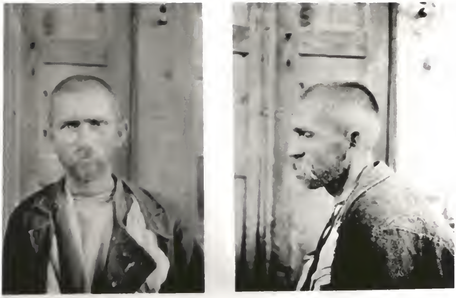
biometric study of different "races of Macedonian men." The study included tables of measurements and statistical analyses of those measurements that calculated standard deviations and coefficients of variation for such traits as cephalic index, shape of nose, body build, skin tint, and color of eyes. The tables and statistics were accompanied by photographs of the various racial pedigrees. To produce such images, the biometricians emulated Alphonse Bertillon's criminal identification photographs (see Figures 9 and 10). The head-on and profile combination, now familiar to us all as the "mug-shot," gave these photographs a professional and evidential character, while the accompanying textual description (Christian Greek, Greek-speaking Mohammedan, etc.) worked to diminish the individuality of the images, to make them photographs of the general object--races or pedigrees. Clothing and grooming revealed a clear class content which, when considered with the text, suggested a biological foundation for class differences.[23]

Initially, the obliteration of the individual in the representation of statistical objects was only partially successful at best, as the individuals photographed stubbornly remained subjects rather than objects. The tension between the objectifying text and the expressions of individuality can be seen clearly in

Christian Greek (Kozani)

Greek-speaking Mohammedan (Chotil).

Figure 9. Photographs from "Measurements of Macedonian Men," Biometrika 21, 1929.



Christian Vlach of usual dark type (Mejidieh).

Christian Vlach of fair type (Samarina).

Figure 10. Photographs from "Measurements of Macedonian Men," Biometrika 21, 1929.

a series of photographs that accompanied a memoir on the "skin color of the crosses between negro and white" (see Figure 11).[24] The text identified each photograph as a representation of the skin-tone that resulted from the "crossing" of pedigrees (negro x white, mulatto x negro, etc.). The effect was undermined, however, by the strong expressions of individuality (dress, posture, facial expression) exhibited by the subjects of each portrait.

Even in the early days, there were some ingenious images, such as the photograph depicting "grades of albinism in Nyasaland natives" (see Figure 12). Here the individual on the left represented (as the text indicated) the "normal native" while the two individuals to his left represented differing degrees of deviation from that norm. In this arrangement, degree of deviation was visually depicted as distance from the norm, as it is on a normal curve.[25]

Over time, the biometricians emulated and reproduced the photographs of a developing "clinical" tradition. In the clinical image, like the ones accompanying a study of "pedigrees of muscular dystrophy" (See Figures 13), all expressions of the subject's individuality were stripped away or controlled, as the process of objectification was taken as far as possible.[26]


Figure 11. Photographs from "A Note on the Skin-Colour of the Crosses between Negro and White," Biometrika 6, 1908-09.

Biometrika, Vol. IX, Parts III and IV

Plate XVIII

Fig. 23. Normal Native, Moyichandi and Sikunda ... of albinism in Nyasaland Natives ...

... and her mother

Figure 12. Photographs from "Anomalies of Pigmentation among Natives of Nyasaland, Biometrika 9, 1913.

*Annals of Eugenics*, Vol. V, Parts I and II
K. Pearson: *Two New Pedigrees of Muscular Dystrophy*

Plate I

IV. 14, aged 32 years; photographs at age 27, provided by Sir J. Purves Stewart.
Facio-Scapular Type of Progressive Muscular Atrophy.

Figure 13. Photograph from "Two New Pedigrees of Muscular Dystrophy," <u>Annals of Eugenics</u> 5, 1933.

## Authorship and the Erasure of the Individual

The rhetorical power of the biometric photographs can only be fully appreciated when viewed in concert with the other rhetorical devices which fashioned the identity of biometry. The craniometry articles in which the photographs of human skulls appeared provide an excellent example.

It is characteristic of articles in Biometrika that they are identified as part of a larger project, and that they created the impression that you have just stumbled into the middle of an argument. The article which contained the 50 photographs of human skulls was no exception. Appearing in Volume III, Part II (1904), the article, entitled "A Study of the Variation and Correlation of the Human Skull, with Reference to English Crania," began by identifying itself as "a contribution to the investigation which has been going on for several years at University College, London, under the direction of Professor Karl Pearson, with the view of determining the size, variability, and correlation of various organs and characters in man."[27]

Next the article alerted the reader to the fact that the text was both a continuation of, and an attempt to extend, "a great paper" that appeared in the first volume of Biometrika. But the article referred to also refused to be an origin, as its title indicated: "A

Second Study of the Variation and Correlation of the Human Skull, with Special Reference to the Naqada Crania." The "Second Study" also identified itself (nearly reproducing the identical phrase) as part of "a general scheme for determining the size, variability, and correlation of the chief organs and characters in man, which has been in progress at University College for some past years."[28] The subsuming of these "individual" contributions into a larger project/text with multiple contributors also extended to their authorship. The editorial style of Biometrika under Karl Pearson downplayed the idea of individual authorship. Pearson's style was to add large portions to, and essentially re-write, the contributions that were eventually published in Biometrika. This fact was always prominently displayed in the footnotes. For example, the exact attribution of authorship in the article in the first volume read: "By CICELY D. FAWCETT, B-Sc., assisted by ALICE LEE, D. Sc., and others, University College London." It was accompanied by a footnote which read:

> I am responsible for the editing and arrangement of Miss Fawcett's material. The present memoir is to some extent a product of cooperation among the biometric workers at University College. On Miss Fawcett, however, by far the most arduous part of the task has fallen. K.P.[29]

The article in volume three concerning English Crania was "by W.R. Macdonell, LL.D.", but the same type

of notation abounded. For example, a note in a section entitled "Material and History of the Site" informed the reader of the (here literal) absence of the author: "Owing to Dr. Macdonell having left London, I have at his request supplied this section....K.P." Nor did the note simply make Pearson the author of the section, as the line of deferral continued, thanking numerous individuals for their assistance: "Mr. Philip Norman, the staff of the Map Department, British Museum, Mr. E.M. Borrajo (of the Guildhall Library), Mr. M. Apted of Stepney, and Mr. W. Minn of Whitechapel."[30] In section 5, entitled "On the Determination of the [Cranial] Capacity,"--a section in which author Macdonell was presumably "present"--there was a footnote written in the first person: "I make this skull 1445 cm.3 and obtain almost exactly 767 grs. from its weight of mustard seed."[31] This note too bore the mark "K.P." In the end, it became impossible and irrelevant to attribute individual authorship to these articles. The individual researcher was erased as the data appeared to produce itself, and was offered directly to the reader as first-hand experience.

<u>Shifting Expertise: The Historical Imperative</u>

The extension of the biometric approach into craniological investigation did not entail the abandonment of typological thinking, rather it required

a shift in the conception of the type as an object of study (from a stable entity which reflected as certain design to a volatile entity whose importance was based on its numerical frequency), and a related shift in the kind of expertise required to identify types with disciplinary authority.[32] In the view of the biometrician, the ability to produce and manipulate statistical data _had_ to replace the experienced eye of the "anatomical craniologist." The evidence for this imperative was to be found in the historical development of science.

Accordingly, the first article on craniometry published in _Biometrika_ sought to establish a historical context which legitimized the biometric foray into craniometry.

> When this scheme was started but little had been done to obtain a scientific measure of the variability and correlation of the parts of the human body. Innumerable anthropometric, including craniometric measurements, had been made and published but very little had been done in determining scientifically their statistical constants. In fact there was considerable danger that the want of proper statistical theory would bring the science of craniology into discredit with archaeologists.[33]

The historical narrative fashioned a tale of craniometry in crisis. In the drama, biometry was cast in the role of modernizing savior. It appeared on the stage at a time when craniometry was in danger of being considered archaic. The only way to bring craniometry

up to date and give it the legitimacy of a modern science was to make sure that craniologists adapted to the demands of an increasingly rigorous scientific environment. Craniologists had to abandon their practice of classifying "a few individuals into different races by means of two or three measurements, such as the cephalic index, the length, or the facial angle,--before the correlation and variation of these characters have been determined for even a single race."[34] Classification of types had to rest not on individual interpretations of the shape and size of a few individual skulls, but on the correlation of characteristics displayed by populations and revealed through rigorous and complete statistical analysis. Failure to do so would turn craniometry into an evolutionary dead end in the progress of science.

The necessity of such a shift and the feathers it was likely to ruffle was explicitly acknowledged. For example, the passage just quoted was accompanied by an asterisk which directed the reader's attention to a footnote. The footnote distinguished between biometry's criticism of primitive craniometry and the long tradition of classification practiced by "anatomists" with keen qualitative powers of discernment formulated over years experience.

> Nothing is here said of the power of distinguishing races which an anatomical

> craniologist may possess after long experience of types. But many such craniologists make their ultimate appeal--and this without the requisite statistical knowledge--to craniometry and not to anatomical appreciation.[35]

A boundary had been established between a tradition of "anatomical craniology" and the future of craniometry. Nothing was said about the older tradition, because nothing needed to be said. The qualitative approach was now situated outside craniometric discourse. Anyone who agreed that a claim to authority must be made on the basis of measurement rather than qualitative appreciation, had already ceded that authority to those with greater statistical expertise. Anyone who disagreed was identified as an outsider.

## Constant Images and Changing Objects

The craniometry articles that appeared in *Biometrika* had a fairly consistent structure. They opened with an introduction that included a historical account of the research leading to publication and how it fit into the ongoing biometric investigation of "Man." Next, there was a description of the material, that is, of the skull collection being examined, including a history of the area where the skulls were found and a discussion of other archeological evidence which might give clues to the nature of the group the skulls represented. This was followed by a discussion

of the measurements made, and of the instruments and methods by which they were taken. Several sections were then devoted to discussions of the various characteristics which concerned the biometrician: the determination of cranial capacity, the degree of homogeneity of the sample, the calculation of mean values for each of the characteristics measured, the determination of the degree of variability displayed by the sample, and finally the correlation of the cranial characters measured. Afterwards, all of the measurements and their relationships were represented analytically and graphically, and surrounded by written texts which guided the reader's interpretation of the data.

The photographs of human skulls permeated, but were not part of, the structure of these articles. Possessing no assigned space (they had no page numbers and the text resumed where it left off, as if uninterrupted by their presence) they were situated, literally and figuratively, at the center of the article. Images of human skulls are powerful signifiers. To the present day reader, these photographs can call up memories of a racialist anthropological discourse which gave an air of scientific credence to the racist doctrines which were used to justify (and perhaps, motivate) imperialism,

colonialism, slavery, eugenics, and the holocaust. The present-day reader does well to view these photographs with intellectual skepticism and moral unease, for biometry participated in the development of that discourse. Indeed, throughout this and other biometric craniometry articles, the main concern was with the identification of true breeding pedigrees--primarily racial types, but also the female, the criminal, and other "deviant" types which all competed for numerical superiority in the general population.

The ideological interests inherent in biometry's construction of these types were often obvious. For instance, in the article by Fawcett in the first volume of Biometrika, one section is devoted to the "Variability of the Naqada Race" (The Naqada race being that supposedly represented by a collection of 400 skeletons excavated from grave-sites in Naqada in Upper Egypt in 1895, and considered to have been interred between 7000 and 5000 B.C.). Fawcett explained that the an analysis of the coefficients of variation calculated for the crania representing the Naqada race, when compared to those values previously calculated for representative crania of other races, showed that the Naqada values lie between those of "`primitive'... and high modern civilization." This result was interpreted as marking "the usual advance in variability with

advancing civilisation."[36] The analysis of the degree of variability in various samples of racial types were said to reveal a hierarchy of development, on top of which the European races invariably appear. This alleged superiority was indicated by the high variability that biometric theory interpreted as evidence of a long period of intense selection; while intense selection was associated with a "high degree" of evolutionary progress. In sum, assertions about degrees of variability were being given meaning by an assumption of progressive evolution, and were then invoked as evidence for the truth of the assumption.

But the viewer at the turn of the century would not have shared our skepticism. What then might the photographs have been expected to connote to the reader of that day? What was their function in the text? Images of human skulls commonly conjured up thoughts of graveyards and ghosts, but connotations of the supernatural were actively discouraged by the photographs in Biometrika. The darkness associated with images of the macabre was replaced by the infinite and antiseptic whiteness that surrounded each image. Even the darkness "inside" the skull was dissipated by the inclusion of photographs of broken skulls which allowed the light (and the reader's gaze) to penetrate the interior and establish that there are no mysteries

inside. The function of these photographs was not dramatic.

The potential of what Sekula has called the "metrical accuracy of the camera" to produce images from which exact mathematical data could be extracted suggests another possibility. This property of photography was noted as early as 1839 when the physicist Francois Arago noted that it was a medium "in which objects preserve mathematically their forms."[37] The series of skull photographs, had they been carefully produced so that each image was to scale, could have functioned as a sample population, providing the reader with the opportunity to check and extend the mathematical data taken from the collection. A process by which the photographs could have been made and reproduced to scale was suggested to Weldon by a fellow craniometrist and occasional contributor to Biometrika, H.H. Turner. "As you are starting on a rather long series of photographs of skulls," Turner wrote to Weldon in October of 1902,

> I will venture to draw your attention at the outset, to the possibilities opened up by simultaneous photographs from different points of view. Briefly my point is that by a little arrangement you may well possibly provide a series of photographs which would enable anyone possessing them to obtain measures of any kind for themselves with very little arithmetic, just as though they had the solid body before them.[38]

Turner then sketched a process by which two cameras photographing the skull at fixed distances would produce images that would yield to mathematical data of certain angles and distances of craniometric importance. Weldon mentioned the process to Pearson calling it a "great method of photography" but also voiced skepticism about it working in practice.[39] Later Weldon reported with some relief that Turner had abandoned the project and returned to the biometric analysis of a skull collection.[40]

In the end, the skull photographs were neither made nor reproduced to scale. The making of the 50 photographs was referred to only in a footnote. The text of the note is very revealing. The first part of the note established that the photographs were made by Pearson and paid for by the Draper's company, the main benefactors of Pearson's research institute. The second section of the note acknowledged that having the photographs to scale would have been desireable, but proved too difficult.

> The scales are here for N. lateralis, N. basalis, and N. verticalis two-thirds natural size, and for N. facialis seven-eighths, but it was found impossible to maintain this accurately throughout the series as the focus must be an average focus, and the control of the engraver could not be complete. After some experimenting with various methods the idea of uniformity of scale was discarded in favour of getting the photograph with the clearest detail.[41]

The ability of the photograph to reproduce the mathematical form of the object proved difficult and was abandoned in order to exploit another more important potential--the potential for detail. What was the value in visual detail?

> In the case of the "special" skulls, the endeavour was made to get the special detail to be illustrated, at the expense very often of the remainder of the object and entirely of scale. In the case of the "normal" skulls, a uniform system of orientation has been attempted, based on the German horizontal plane, the auricular vertical plane, and the median plane of the skull. If however, the skull supports are not to interfere with the effectiveness of the photograph, this orientation will not be absolutely exact.[42]

The uniformity of scale that would have produced biometrically useful (but also redundant) images was abandoned so that optimal focus would produce images replete with sufficient detail. Detail which brought home the reality of the crucial differences that define the sub-groups within the sample population as identified by statistical analyses. In all cases, concerns for scale were sacrificed if they interfered with the "effectiveness of the photograph." It was not, it would seem, Turner's notion of creating an archive of images that would yield accurate measurements that the biometricians found useful, but rather his point about photographs having an inherent ability to make the reader feel "as though they had the solid body before

them." In short, the editors of Biometrika produced photographs not for the faithful, but for the skeptical.

A perusal of the presentation of the skull photographs illustrates the point. Each photograph depicted a single skull, arranged in one of five positions, or providing one of five viewpoints (see Figure 14). In each, the skull was positioned in the center of the page and suspended in an empty boundless white space.[43] Black typeset copy in the upper left hand corner identified each representation as the property of Biometrika, and located each in space and time--"Vol. III., Part II." Beneath each representation a different script--one which gave the appearance of being hand-written--identified each depicted skull in terms of its normality, gender, and ontology (e.g., "Normal Male Skull"), and marked it as a representation of an individual object (e.g., skull "W. 45").

A smaller script, directly underneath, named, in the case of the normal skulls, the position of the skull, or vantage point provided, by the photograph. The script read "N. facialis" when the viewer confronted the skull directly--face to face. In each of these encounters, the viewer was confronted with both a presence and an absence. At first glance, the viewer's gaze seemed to be returned as the outlines of a "face" looked back from the page, but the eye sockets were


Figure 14. Photograph from "A Study of the Variation and Correlation of the Human Skull, with Special Reference to English Crania," Biometrika 3, 1904.

empty--it was only an object, the text reassured the viewer ("Normal Male Skull"). The gaze was unreturned and the viewer could we probe the object at will.

The second view in the series was labeled "N. lateralis".[44] Here the image was rotated 90 degrees so that the skull appeared in profile. A second 90 degree rotation counter-clockwise had also occurred, so that the contours of the skull matched those of the page. The move was no doubt made so that space was used efficiently and tidy margins were kept. However, the text was also rotated so that the viewer was compelled to negate the second rotation, turning the page 90 degrees clockwise. In the process, a distinction was made and enforced: the first move was a rotation of the object, the second move, which was to be negated and forgotten, was merely a rotation of the image. The relationship between object and its representation was, as we shall see, crucial to the substitution.

The first two positions or viewpoints, frontal and profile, recalled Bertillon's identification photographs. The third viewpoint in the biometric series took the viewer beyond them, to a place where a photograph of a living person could not go. "N. basalis" revealed the base of the skull, seemingly suspended in the air face down, revealing the gaping hole of the forum magnum. The fourth image in the

sequence was labeled "N. verticalis." This view reversed that provided by N basalis to depict the top of the head. The final position, termed "N. occipitalis," returned the object (and/or the viewer) to the orientation of N. facialis, but the object was rotated 180 degrees, or the viewer has moved behind it.

In the context in which Pearson and Weldon launched Biometrika, scientific authority flowed from communal recognition of, and deference to, a given set of expertise. Recognition and deference hinged upon the ability of a given mode of representation to provide the experience of objective, unmediated observation. Both the biometricians' expertise and their mode of representation were unfamiliar to the extremely heterogeneous audience that comprised the ill-defined community of human and social scientists at the turn of the century. The first step in gaining acceptance was to answer the criticism that their esoteric statistics did not refer to any particular objects in nature. Viewed in sequence, the skull photographs functioned rhetorically to recreate the experience of first-hand observation of the object, as if the viewer was alone with the skull, rotating it in order to examine it.

The next step involved the demonstration that traditional craniological description could be fully translated into, and then superseded by, the new and

more powerful biometric analysis. It needed to be demonstrated that knowledge about racial types gleaned from the qualitative analysis and represented by skull collections could be translated into, and superseded by, knowledge gleaned from quantitative analysis and represented by mathematical curves. The initial rhetorical move was to show that the skulls and the curves could be made to inhabit the same space--namely the two dimensional page. The skull photographs provided the vehicle for the alleged translation. The photograph claimed to apprehend the three dimensional object and represent it in the two dimensional space. The photograph mediated the difference between the second and third dimensions. Later, the biometricians used photographs to demonstrate the reciprocity of the translation, producing photographic images of mathematical curves which inhabited three dimensional space (see Figure 15).[45]

The skull photographs served no biometrical function. The biometric acolyte had no pressing need for them. The photographs functioned to lure the skeptic. They invited the traditional craniologist into a translational discourse about racial types in which the reader could apparently choose to consider the problem either qualitatively, through the represented skulls, <u>or</u> quantitatively, through the statistical

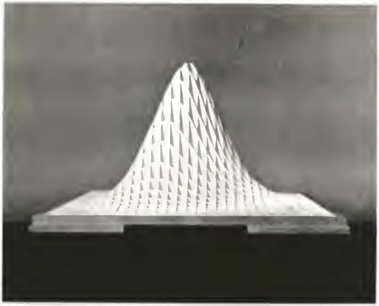


Figure 15. Photograph of models of mathematical curves published in Volume 5 Biometrika, 1906-07.

material. The invitation, however, turned out to be at best a seduction. For translation was not what was offered, rather a subtle sleight of hand in which the object of the traditional craniologist was removed, forever, in favor of a representation which did not, as it turns out, even attempt to "stand for" the skulls. For in a footnote in the very section of the article in which the invitation was extended, the biometric view of the strengths and weaknesses of the photographs was stated: "On the whole we believe that the real and invaluable advantage of photographic reproduction is to show the indescribable in type and abnormality; it cannot be properly used to replace an elaborate system of individual measurement."[46] Far from representing the absent artifact--the material skull, the photographs merely failed a little less than words in the attempt to translate the untranslatable: the precise knowledge of type and abnormality produced by statistical analysis and articulateable only in the language of statistics.

The rhetorical functions of these photographs was indicative of a strategy of substitution in the guise of translation that characterized biometric incursions into established discourses. What were seemingly translated were the relations between an object and modes of representation, between a skull and photographic and statistical representation. But it was not so simple.

In traditional craniological discourse, the photograph would have represented an absent individual skull. In biometric discourse the photograph represented something very different. Consider the fact that all of the skulls in the series were not photographically reproduced. A selection had to be made. The author of the article described the criteria by which the selections were made:

> In selecting normal crania, for reproduction, we have been guided by the desire to show not only "modal" individuals, but individuals at some distance from the mode on either side....We have endeavoured to give in these "normal" crania, some conception of the skulls of this English series.[47]

In biometric discourse, the photographs of skulls represented <u>not</u> an individual object, but the aggregate object represented by the normal curve. The translation of traditional and biometric discourses about crania was an illusion. The two were translatable <u>only</u> if there was agreement that the objects of study were the aggregate objects of statistics. A discourse that commences in agreement requires no translation.

## <u>Science, Knowledge, and Representation</u>

Examining the role of statistics and photography in the development of biometry, it becomes exceedingly difficult and disingenuous to maintain that the objects of biometric science were found and intervened with only to be represented after the fact. Instead, the natural

objects of biometry, and of the other components of the turn-of-the century search for true human and social science, were fashioned within developing modes of representation which both demanded and produced them. Those modes of representation had meaning and value within specific and related historical contexts. The observational rigor and assumed objectivity of statistics and photography were valued in the context of a movement to remake biological science in general, and the human and social sciences in particular, in the image of the physical sciences. The desires that motivated that movement, and the ability of statistics and photography to satisfy them, must be understood within the larger context of the emergence of new institutions and new practices of observation and record-keeping, which were themselves bound up in a restructuring of the relationship between individuals and the institutions of the state.

It must also be recognized that the slow transformation within the human and social sciences of the concepts of normality and deviance--a transformation both demanded and fulfilled by statistical and photographic modes of representation--had tremendous consequences for flesh and blood individuals. The "normal type" as conceived by Quetelet and Galton had a comforting resiliency. In their view, the existence of

deviant types was unfortunate, but they believed that the ideal type that embodied the character of the nation would always exist. In the thought of the biometricians, the fitness of the national stock became an insatiable desire, while deviation from the desired norm became a dire threat. Individuals like those in the photographs shown in this chapter were subjected to increasingly intrusive observation and forced to "stand for" dangerous deviance. They bore the burden of being made into the objects of the biometricians' desire and dread.

Finally, all of this was carried out on the basis of a still prevalent metaphor which compares modes of representation to a mirror held up to nature. But the metaphor is flawed. Neither statistics nor photography work like mirrors. Both are processes of intervention and manipulation by which new and specific realities (statistical graphs and photographs) are produced. Further, the meaning and value of those new realities are contingent upon the specific historical context of the discursive or representational systems of which they are part. The images produced by statistics and evidential photography exercise authority only when we desire what they offer--the illusion that the "problem" of the individual can be obliterated.

## Notes

1. See, for example, Robert Darnton, The Great Cat Massacre: And Other Episodes in French Cultural History. (New York: Vintage Books), 1985.

2. John Tagg, The Burden of Representation (Amherst: The University of Massachusetts Press), 1988. pp. 37-44; and Allan Sekula, "The Body and the Archive," October 39 (1986): 3-64. pp. 4-5.

3. Tagg, Burden of Representation, pp. 48-57.

4. See Tagg, The Burden of Representation, and Sekula, "The Body and the Archive." Both analyses rest on an interpretations of Michel Foucault's metaphorical use of Jeremy Bentham's "Panopticon." See Michel Foucault, Discipline and Punish (London: Allen Lane), 1977.

5. Tagg, Burden of Representation, p. 74.

6. Sekula, "The Body and the Archive," p. 5. See also, Gareth Steadman-Jones, Outcast London (Oxford: Clarendon Press, 1971).

7. Sekula, "The Body and the Archive," pp. 10-13. For physiognomy see also, Judith Wechsler, A Human Comedy: Physiognomy and Caricature in Nineteenth-Century Paris (Chicago: University of Chicago Press), 1982. For phrenology, see David de Guistino, Conquest of Mind: Phrenology and Victorian Social Thought (London: Croom Helm), 1975.

8. Sekula, "The Body and the Archive," pp. 13-14.

9. Tagg, Burden of Representation, pp. 77-81.

10. Tagg, Burden of Representation, pp. 82-87.

11. Theodore M. Porter, The Rise of Statistical Thinking (Princeton: Princeton University Press, 1986), p. 291; Sekula, "The Body and The Archive," pp. 19-28. The use of photographic techniques and the creation of a multi-layered identification system in the British context followed the adoption of Sir Edward Henry's system of identification by means of fingerprints (which had also been the subject of much work by Galton) by New Scotland Yard in 1901. See Tagg, Burden of Representation, p. 75.

12. See Chapter 5.

13. Galton first created the composite photograph in 1877. He offered a complete description of his initial process for producing them in Inquiries into the Human Faculty and Its Development (London and New York: AMS Press, 1973, Reprint of the 1907 edition), pp. 221-241. For another example of the use of composite photography in the human sciences (in this case in the work of the psychiatrist Adolf Meyer), see Ruth Leys, "Types of One: Adolf Meyer's Life Chart and the Representation of Individuality," Representations 34 (Spring 1991): pp. 1-28.

14. Galton, Inquiries, p. 7.

15. Ibid., p. 10.

16. Ibid.

17. Sekula, "The Body and the Archive," p. 18.

18. Ibid.

19. Ibid.

20. Ibid., p. 19.

21. Tagg, Burden of Representation. Especially Chapter 3, "A Means of Surveillance: The Photograph as Evidence in Law," pp. 66-102.

22. For a full treatment of the development of a realist discourse in visual representation in the nineteenth-century, see Jonathan Crary, Techniques of the Observer: On Vision and Modernity in the Nineteenth Century (Cambridge, MA: MIT Press), 1990. Also see Roland Barthes, Camera Lucida (London: Johnathan Cope), 1981; especially pages 76 and 87. In this work Barthes, exploring the phenomena of loss and grief, becomes an unlikely spokesperson for the realist position. Barthes argument is analyzed and rebutted in Tagg's "Introduction" to Burden of Representation.

23. Margaret M. Hasluck and G.M. Morant, "Measurements of Macedonian Men," Biometrika XXI (1929): 322-336.

24. Karl Pearson. "A Note on the Skin-Colour of the Crosses Between Negro and White," Biometrika 6 (1908-09): pp. 348-352.

25. Hugh Stannus Stannus, "Anomalies of Pigmentation among Natives of Nyasaland," Biometrika IX (1913): 333-365.

26. Karl Pearson, "Two New Pedigrees of Muscular Dystrophy," Annals of Eugenics 5 (1933): 179-191.

27. W.R. Macdonell, "A Study of the Variation and Correlation of the Human Skull, with Special Reference to English Crania," Biometrika 3 (1904): 191-244. p. 192.

28. Cicely D. Fawcett, "A Second Study of the Variation and Correlation of the Human Skull, with Special Reference to the Naqada Crania," Biometrika 1 (1902): 408-467. p. 408.

29. Fawcett, "A Second Study of the Variation and Correlation of the Human Skull," p. 408.

30. Macdonell, "A Study of the Variation and Correlation of the Human Skull," p. 193.

31. Ibid., p. 204. The packing and weighing of mustard seed was a common method of determining cranial capacity. See Stephen J. Gould. The Mismeasure of Man (New York: Norton, 1981).

32. Ernst Mayr has suggested that the type of population thinking that accompanied the statistical approach was the beginning of the end for the kind of typological thinking that he terms "Platonic essentialism" in the life sciences. See E. Mayr, The Growth of Biological Thought (Cambridge, MA.: Belknap Press, 1982).

33. Fawcett, "A Second Study of the Variation and Correlation of the Human Skull," p. 409.

34. Ibid.

35. Ibid.

36. Fawcett, "A Second Study of Variation and Correlation in Human Skulls," p. 412.

37. Francois Arago, letter to Duchatel, in Helmut and Alison Gernshiem, L.J.M. Daguerre (New York: Dover) 1968), p. 91. Quoted in Sekula, "The Body and the Archive," p. 17.

38. H.H. Turner to W.F.R. Weldon, 14 October 1902, Folder 266, Envelope 10, Karl Pearson Papers, University College London Library, London. The emphasis is Turner's.

39. Weldon to Pearson, 2 October 1902, Folder 891, Karl Pearson Papers, University College London Library, London.

40. Weldon to Pearson, 26 October 1902, Folder 891, Karl Pearson Papers, University College London Library, London.

41. W.R. Macdonell, "A Study of the Variation and Correlation of the Human Skull," p. 215.

42. Ibid.

43. The effect seems to have been created by cropping the image. In a few of the photographs, the effect has been neglected and the table and other props are visible.

44. The various positions-viewpoints are always presented in this sequence, but the full sequence in not always produced for each skull represented.

45. Actual physical models of mathematical curves were built in three dimensions and then photographically reproduced on the two-dimensional page.

46. W.R. Macdonell, "A Study of the Variation and Correlation of the Human Skull, p. 215.

47. Ibid., pp. 215-216.

# CONCLUSIONS

History and biology enjoy a privileged position in the production of knowledge about human beings. It is, therefore, important that we understand how knowledge is produced in the practice of these two disciplines. The process of writing is fundamental to both, and the knowledge produced within historical and biological discourse is fictive, not in the sense of "made-up," but in the older sense of "fashioned." The identities of the researcher and of the objects researched are fashioned, along with the knowledge that exists in the imagined space between the two, within a context created by metaphor and narrative.

The diverse investigations that constituted biometry between roughly 1890 and 1932 were part of a research program with reformist aspirations. That program had coherent meaning to the biometricians because it participated in a larger narrative of Darwinian evolution. Recently, Misia Landau analyzed a number of historically significant evolution narratives, identifying a common structure and comparing that structure to that of the fairy tale.[1]

Landau summarizes the narrative structure of Charles Darwin's tale of evolution in the following way. The story is set in some warm forest clad-land, where a weak and nearly defenseless hero lives in the trees. Some manner of environmental change forces the hero to descend from the trees and to depart on a long journey. The journey is a passage from a situation of safety and prosperity to another of scarcity and danger. This change in setting transforms the hero, who becomes "more and more erect," removing him from his arboreal ancestors. The new, more dangerous setting tests the hero, pitting him against all manner of creatures in a battle of wits. Aided by a donor, natural selection, the hero does well in the battle of wits and is transformed in the process into an intelligent and almost human creature. Reaching the stage of civilization, the hero is tested again by the conflict of the individual will and the will of the group. By developing his moral sense, the hero triumphs over lower men and his own lower nature. Having concluded his journey, Darwin's hero is left facing an uncertain future.[2]

The meaning of biometry, the identity of the biometricians, and the identity of the objects of their research, were all fashioned within that narrative. But biometry is almost exclusively concerned with the last

act of the story. The biometric hero--the English Man--has developed a technologically advanced, morally aware society, but his achievement is threatened by hubris; he has put himself beyond the reach of Darwin's donor, natural selection. In order to continue to evolve and improve, and to preserve the civilization he has built, he must become his own donor; he must take control of his evolution.

The central themes of that narrative were repeated and illustrated in the biometric histories and in Pearson's biographical account of Weldon's life that served as a model for the evolution of a biometrician. What biometry added to the narrative was a sense of urgency. Constituting the norm as a constantly contested value on a continuum of difference, the authors of biometric discourse fashioned a present moment when undesirable deviance was threatening to define the normal characteristics of the English race and nation.

It was in that context that the identities of the biometric researcher and of the objects of their research were identified in opposition to one another. The biometrician was identified as a pedigree of desirable traits, rationality, health, and self-discipline. The objects that the biometricians produced knowledge about were identified as pedigrees of

undesirable traits, insanity, disease, and criminality. The true breeding pedigrees that biometric science measured were reifications of categories of difference that the biometric narrative demanded and produced. Traces of the social tensions inherent in those categories are visible yet in the juxtaposition of the self-portraits of the biometricians (see Figure 16) and in the photographs that illustrated their science (see Figure 17).

Figure 16. Portraits of Karl Pearson, published in Biometrika 28, 1936.

K. Pearson: *Two* [illegible] *Muscular Dystrophy*

Fig. 1.

Fig. 2.

G. B. Duchenne's case from the *Album de Photographies pathologiques*, 1862 (Figs. 1 and 2). Progressive muscular atrophy of the facio-scapular type in a youth. Note the droop of the lips, the projection of the scapulae, the condition of the arms and legs and the lateral curvature.

Figure 17. Photographs from Pearson's "Two New Pedigrees of Muscular Dystrophy, <u>Annals of Eugenics</u> 5 (1933).

## Notes

1. Misia Landau, Narratives of Human Evolution, (New Haven/London: Yale University Press, 1991). Specifically, Landau analyzes the evolution narratives of T.H. Huxley, Ernst Haeckel, Charles Darwin, Arthur Keith, and Grafton Elliot Smith. She then provides much shorter treatments of some of the current paleoanthropologic literature.

2. Landau, Narratives, pp 46-54.

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Jeffrey C. Brautigam was educated in the public school system in the state of Florida. Upon graduation from high school, Jeff was employed in the television production industry.

In the Summer of 1980, he came to the University of Florida in pursuit of a liberal arts education. Seduced by the challenges offered by the history of science, Jeff enrolled in the interdisciplinary program in the history of science, technology and medicine, and received a B.A. in May of 1984. In 1987, he completed an M.A. in the history of science, writing a reconsideration of Fleeming Jenkin's critique of the Origin of Species.

Jeff taught European history at Georgia Southern University during the 1992-93 academic year, and will begin teaching British history at Temple University in the Fall of 1993.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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